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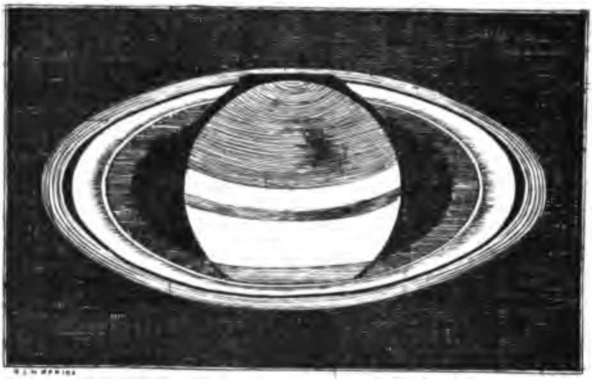
ENGRAVINGS.

Notices to Correspondents, &c., &c.

SATURN.



Observed 9h. 30m., Dec. 5, 1884, by Mr. B. J. Hopkins
with an 18-inch reflector and power of 1,000.
Definition middling.

The Astronomical Register.

No. 265. JANUARY. 1885.

ROYAL ASTRONOMICAL SOCIETY.

Session 1884—85.

The December meeting was held at the Society's Rooms, Burlington House, on Friday, Dec. 12th, 1884.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—Col. Tupman, and Mr. A. M. W. Downing, acting for Mr. Knobel.

The following gentlemen were balloted for and duly elected Fellows of the Society:

H. P. Hollis, B.A., Royal Observatory, Greenwich, S.E.
Thos. Hovendon, Selhurst Road, South Norwood.
Thos. Lewis, Royal Observatory, Greenwich, S.E.
Lord Maclaren, Edinburgh.
Rev. W. F. Baden-Roome, The Manse, Aldershot.
Harold Seward, B.A., Balliol College, Oxford, and 2, Normanby Street, Liverpool.
Rev. Henry Wheaton, M.D., 9, Star Hill Terrace, Rochester.
Rev. J. Wilkins, 16, Barforth Road, Peckham Rye, S.E.

Colonel Tupman reported that 52 presents had been received since the last meeting. Amongst them was one calling for special notice,—the second volume of Dembowski's Double Star Observations, consisting chiefly of papers reprinted from the *Astronomische Nachrichten.*

A vote of thanks was passed to the donors.

Mr. Talmage proposed Mr. Lecky, Mr. Inwards, and Mr.

Maunder, for election as auditors of the accounts of the Society, to be presented at the February meeting.

Sir James Cockle: I have much pleasure in seconding that motion.

The motion was put and carried.

Mr. Edmund Spitta read a paper on a *New Occulting Eye-Piece.* He said: I do not claim any novelty for the idea of hiding a bright object behind a bar or band while examining the fainter parts around, but simply for the means devised for that purpose, which has proved successful, convenient, and economical in construction. It consists of a small brass cylindrical box with four shutters placed in front of a tube carrying the lens. The box is about a quarter of an inch deep, and one and a half inches in diameter. I found some difficulty at first in making the shutters, so that they would not collide, and yet preserve their edges in focus; but this was at last accomplished by bending two of them in deep curves, and allowing the others to work in the curves so formed. By using one screw the field is occulted in the corresponding part, but by manipulating the four it can be narrowed down to a slit, or minimised to a small square the size of a pin-hole. Owing to each shutter being at right angles to its neighbour, the field can at any time be made into an arc of 90°, so that the instrument affords a convenient means of approximating position angles. I examined with this instrument attached to my 10 inch, a faint point of light near *Alpha Sagittæ*, and Mr. Burnham also examined it with his large refractor. I fixed the position angle at 180°, and Mr. Burnham gave it as 180°·5, so that there is not much difference. It might be argued that a bar and an ordinary eye-piece would answer the same purpose, but that requires a good clock to keep the bar in place, and then the glare is seen on both sides, but with this instrument it *can* only be on one side, and there is no loss of time in adjustment, as it revolves on its axis, to say nothing of having two powers. There are some who may advocate a Dawes's eye-piece as an occulting agent, but this is apt to occult the part that it is desired to observe. The special uses for which this instrument was designed are—searching for faint companions to bright stars; examining the satellites of the Planets, especially those of Jupiter in transit; details of the moon's surface, and searching for faint comets and nebulæ.* I have to express my

* [Note by Mr. Spitta.] Possibly this instrument may be of service in celestial photography by enabling the intense parts and the faint surroundings to be separately photographed. Provision was made for this in the manufacture of the instrument, for by loosening two screws two of the shutters come away, and others of special shape can be substituted.

thanks to Mr. Mason for overcoming some difficulties in the making of the instrument, and to Mr. Wray, junior, for designing the Ramsden eye-pieces of such high power.

The President: I am sure we must thank Mr. Spitta for his paper, and the beautiful eye-piece he has produced; so far as my knowledge of the instrument goes it may be very useful. We have a long paper from Mr. Gill on *Systematic Errors in the Reading of the Circle Microscopes of the Cape Transit Circle.* Mr. Downing will read some portions of the paper, which is too long to read in full.

Mr. Downing read the general conclusions at which Mr. Gill had arrived: 1. That the screws of transit circles require much more frequent examination than it has been customary to give them. 2. That steel screws should be substituted for the gun-metal screws in the Cape circle microscopes. 3. That the screws in the microscope micrometers should be reversed as at Greenwich, so that the springs may press the bearings of the screws in the opposite direction. 4. The N.P.D. results of the Cape catalogue for 1880 must be corrected for screw errors, and the latitude determination be re-discussed, before they can be admitted into fundamental determinations.

The President: I may say that the wearing of the screws of the transit circle has caused a great deal of trouble at the Greenwich observatory. You will remember that Mr. Christie has published a paper showing the effects of the systematic errors of the screws, the consequence of which has been that the micrometer screws have been so arranged that the effect of the wearing of the screws in future will be prevented. Of course, in the case of meridional observations it is highly important that all these errors of the instrument should be found out and corrected. We look upon Mr. Stone's great catalogue of 12,400 stars as a great work, and I cannot help thinking that great praise is due to him for the energy he has shown during the ten years he was at the Cape, and I think our knowledge of the places of the stars in the southern hemisphere will for some considerable time to come be due very much to the labours of Mr. Stone. Then it appears that Mr. Gill, since he has been at the Cape, has discovered that the screws of the Cape circle, which is an exact copy of the Greenwich transit circle, and made by the same makers, Ransomes and May, of Ipswich, and Troughton and Simms, are also wearing away, and that to that cause is due certain systematic errors. Mr. Stone knows that errors of this kind must creep in, and that it is impossible to produce such an important catalogue as he has done without its containing some systematic errors.

Mr. Stone said: The Cape catalogue for 1830 must speak for itself, and I have no fear but that it will stand future tests as it has stood the past. Mr. Gill's assumptions of error will not make the work less accurate, although they may for the time create false impressions as to their magnitude. With respect to the screw errors, our determinations of nadir point are merely stepping stones to the polar distances, and even if they were affected by a constant error this would not affect the polar distances when the colatitude correction is deduced from observations above and below the Pole. Such constant errors, if they existed, would, however, be clearly shown in the colatitude determinations, and no such errors are indicated. I did, however, whilst at the Cape, check from time to time the working of the instrument as a whole over the entire range of screws used in the work, and I proved that the errors over the range used were exceedingly small. But I did show that if we had extended the use of the screws much beyond the range of 5′ actually required in the work, then the passing accurately from one five-minute division to another, which is all we require, was lost; and, as a practical man, I therefore ordered the observations to be restricted to a range of screw within which I could prove that the errors were small. This nadir check is completely conclusive on such a point. If you can take your nadir readings without greater errors than those which you are prepared to accept over the whole range of five minutes, then your observations will not be affected with instrumental errors due to the screws of greater amount. But I checked the same thing from time to time by observations of a collimating mark. So far, therefore, with respect to these screw questions; small errors undoubtedly did exist, but they were very small, and in mean results must have been practically insensible. With respect to wear, what does Mr. Gill mean by wear? I speak only of irregular wear such as prevents the readings over 5′ being referred correctly to each other. I most certainly found, on examination of the screws, that the threads near the head were not, as a matter of fact, finished like those near the central part of the screw. Mr. Gill's curve—which is a reproduction of one I prepared years ago—shows clearly enough the general flatness of the field for readings over the parts of the screws generally used, and the rapidity with which the errors change when the readings are taken near the ends of the screws, and this I most certainly consider due to what I have called "strain" when the readings change per saltum at the extreme limits of the screws. With respect to the existence of such errors, if the range of the screws be unduly extended in the earlier observations, 1856, when the screws were new. I

stated that they did exist from an examination of the records, and I have not the slightest doubt my statement was founded on fact. Mr. Gill's arithmetical discussions of screw corrections appear to me to be waste of labour; he assumes that the zero point from which revolutions are counted has always corresponded to the same part of the screw, but I know that such is not the case, for the index for revolution and the object-glasses of the microscopes have been adjusted from time to time; and I certainly have altered the revolution corresponding to 0^r in order to avoid the use of threads which did not work regularly with those in regular use. If any serious errors affected the nadir readings they would be shown in the colatitude determinations.

The following may be referred to:—

From the observations, 1870-1879, I found	$123^\circ\ 56'\ 3''.41$
„ „ 1856-1860, „	$123^\circ\ 56'\ 3''.20$
Whilst Henderson found from the mural circle observations, which instrument occupied nearly the same position as the transit circle	$123^\circ\ 56'\ 3''.56$

There are here no indications of serious errors.

Mr. Downing read a short paper *On the right ascensions of certain standard polar stars,* by Prof. Safford, who advocated the more systematic observation of stars of high northern declination. Some researches had been made, but they were for the most part unpublished. It would be desirable that a larger number of these stars should be inserted in the star catalogues of the principal observatories.

The thanks of the Society were accorded for the paper.

Colonel Tupman exhibited an elaborate chart by Mr. Marth, and explained that it was a sort of projection of the orbits of all the planets and some comets on a revolving plane. He had no doubt the Fellows of the Society, as soon as they examined it, would understand and comprehend it. (Laughter.) Unfortunately Mr. Marth has not sent a description of it. There are three sheets, which form a continuous chart. It is really a projection of the orbits of all the planets, including the minor planets, on a revolving plane perpendicular to the ecliptic; every orbit will describe a curve on that plane as the latter rotates.

Mr. Ranyard: I think it is not a projection, but the curves are traced out by the point of intersection of the orbits with the revolving plane.

Colonel Tupman: Yes; they are traced by the intersection of the orbit with this revolving plane.

The President: There is nothing here to explain it but a series of figures.

Mr. Christie: The object is to show the eccentricities of the orbits in combination with the distance from the sun and the inclination to the ecliptic.

Mr. Common: As far as I understand it, Mr. Marth's idea may be stated thus: If a plane at right angles to the ecliptic revolves upon an axis passing through the sun, the trace corresponding to a planet moving in a circular orbit in the plane of the ecliptic will be a dot: for, if the circular orbit is concentric with the sun, it will always intersect the revolving plane in one point. If the orbit is elliptic and in the plane of the ecliptic, we shall have a straight line at right angles to the axis, about which the plane revolves, and the length of the line will show the eccentricity of the orbit, and the two ends the perihelion and aphelion distances. If the plane of the orbit is inclined to the plane of the ecliptic, and the orbit is elliptic, the trace will be a curve more or less symmetrical, which from inspection enables you to determine the chief data of the orbit.

The President: Perhaps we had better adjourn this discussion till Mr. Marth has given us his description, which I believe he has promised to do.

Colonel Tupman: We have a long paper from Mr. Denning on the *Long Duration of Meteoric Radiant Points*, which is an exceedingly interesting paper to those specially interested in the subject. It contains the results of a vast amount of observations made during many years by Mr. Denning, who had previously found that certain radiant points of shooting stars lasted for several months. He demonstrated the duration of these radiants by tracing them independently from week to week, so that he gradually grew to the conviction that these radiant points were really persistent at definite points for months together. He communicated his ideas to Professor Herschel and others, but they had a difficulty in reconciling such phenomena with theory, because if such meteors belonged to systems with parallel paths, and the radiant points remain stationary for three months, it would prove that the sun had no power to influence their orbit. However, Mr. Denning set to work, notwithstanding this rebuff, and has made a very long series of observations, carefully and well, and has now demonstrated from them, and from the observations of others also, that a great number of radiants last for several months. There is no sort of doubt of the existence of these radiants. What the explanation is I do not know. I will copy one of Mr. Denning's charts on the black-board, so that you may see what sort of results he gets. The evidence that Mr. Denning produces is overwhelming. I am very much struck with the accuracy with which his radiants are determined. He

rejects all doubtful observations, and thus gets the position of the radiant which cannot be more than a degree or so in error.

Mr. Ranyard: There is another point which Colonel Tupman has not referred to. These little groups of radiants, which Colonel Tupman has been drawing on the black-board, correspond in every case, Mr. Denning says, with meteors of the same type. Meteoric observers know that the meteors coming from any radiant have a family likeness; some of them come at a swift rate, some are coloured, some leave streaks. A great many little peculiarities can be observed which together constitute the family likeness. The meteors coming from the little groups Colonel Tupman has described all exhibit the same peculiarities, which is a strong reason for supposing that they come really from the same radiant point, and not merely from a series of radiants which happen to be a degree or two apart. I have asked Mr. Denning with what accuracy he can determine the radiants. Of course there are some which can be determined with greater accuracy than others, He thinks that under favourable circumstances they can be determined within a degree of error. If particles are seen to come from the same radiant within a degree for six months, it means not only that they must be moving parallel to one another in space, but that the different direction of the earth's motion on opposite sides of its orbit does not make a difference of one degree in the direction in which the particles appear to arrive. This would mean that they must be moving at a very great rate. A shift of one degree on either side of a mean radiant point would correspond to a velocity of the meteors in space fifty-three times as great as the earth's orbital velocity. Now, it is certain that the meteors from fixed radiants do not disappear in a fifty-third of the time in which meteors from the known planetary groups are consumed in the earth's atmosphere. For example, we know pretty exactly that the meteors from the great November shower meet the earth with a velocity of 44 miles a second. But theory would require that these meteors should meet the earth with a velocity of fifty times 18 miles a second, that is, with a velocity twenty times as fast as the November meteors. Even the November meteors rank as swift. Mr. Denning states that the meteors from his fixed radiants are all swift; but they do not disappear in a twentieth of the time occupied by the November meteors. But this does not necessarily imply that the meteors from the swift radiants do not enter the earth's atmosphere with hyperbolic velocities twenty times as fast as the November meteors. We know very little accurately at present about the height at which small meteors become incandescent and begin to attract

attention. It is possible that they remain incandescent, and are not absolutely burned out till the central part has lost a large proportion of its initial velocity. The swifter meteors would certainly become incandescent at a greater altitude than the slower moving particles, and, if our attention is not attracted till they have lost a large proportion of their original velocity, it is possible that we might not recognise the great difference of velocity between the meteors belonging to planetary groups and those coming from fixed radiants. Mr. Denning does not give any of the fixed radiants as lasting the whole year round, but this is because such radiants cannot be observed when they have the same, or nearly the same, right ascension as the sun, unless they were near to the Pole, and he has not yet detected any such radiants in the circumpolar regions. Mr. Denning has an idea that his fixed radiants might possibly correspond to groups of meteors moving in orbits round the earth; but there are great difficulties in such a theory; it is not easy to see how they could ever plunge into our own atmosphere unless their perigee distance were liable to be changed by the moon, or the attraction of some other body outside the earth; but, if the perigee distance were changed, it is probable that the other elements of the orbit would be disturbed, and then the meteors would appear to come from a different radiant.

Mr. Waters: Is there no sort of explanation of this? Is it not possible that these meteors accompany the earth, and at some point of their orbit they enter the atmosphere at the nearest point?

Colonel Tupman: That is not likely, because any such system would be very quickly exhausted. Supposing in perigee a stream to enter the earth's atmosphere, such a stream would be exhausted in a single revolution. There could then only be one display. If they entered the atmosphere they could not get out again.

Mr. Waters: Might not a fresh supply come in from some outside place?

Col. Tennant: How long a period have these observations extended over?

Col. Tupman: I think about fifteen years or more.

Mr. Chambers: Observations of the same point year by year?

Col. Tupman: I believe it is Mr. Denning's opinion that if you were to put all his observations in the fire and begin again and go on for another fifteen years you would be precisely in the same place as now, that these radiant points would be clustered very near together, only differing in position or by the probable error of the determinations.

Mr. Rand Capron: One point was mentioned by Mr. Ranyard, that these showers have independent characters, with respect to their brightness, their continuance, and leaving trails of light behind, and so on. It seems to me important to know, if we can, whether these peculiar characteristics have their corresponding spectroscopic qualities. I am perfectly aware how difficult it is to obtain this with the spectroscope; but it has been done to some extent, and the results published in Hungary; and Mr. Browning and some others have made here observations of the kind. There is a good deal of difficulty, of course, in the case of meteors which are of such a transitory character. There is a great deal of care required in getting such a spectrum, but it seems to me that in solving the questions of what these meteors are, to what class they belong, and where they come from, it would be important to get something like a spectroscopic definition of particular classes of meteors; for naturally it follows that if a meteor leaves a trail of light behind it there would be some phosphorescent spectrum connected with it, or if the meteor is of a yellow tint, we might reasonably suppose characteristics belonging to the yellow portion of the spectrum to make their appearance. I have heard nothing of the spectroscopic character of the meteors, and do not know whether Mr. Denning has obtained any spectra.

Mr. Ranyard: It is almost impossible with small meteors.

Mr. Rand Capron: But with large ones, I take it, it would be possible. It seems to me that if gentlemen who have the opportunity of observing these meteors would bear in mind the spectroscopic question, it might be an important point in settling some of the questions affecting their character.

Mr. Maunder: Has Mr. Denning found any radiants behind the earth, *i.e.*, in the part of the heavens from which the earth is proceeding in its orbit?

Colonel Tupman: Yes, I think there are a few; you cannot expect to have many, but there are some.

Mr. Maunder: Has any difference in the character of the meteors been noticed when the earth is proceeding towards the radiant and when it is receding from it, because, of course, there will be a difference of thirty-six miles a second in the rate at which such meteors would meet the earth.

Colonel Tupman: It will make a difference in the velocity, clearly. The meteors that overtake the earth are reddish in colour. Nearly all the radiants that come from what is called the anti-apex of the earth's way are of a reddish tint, and they are of very slow velocity.

Mr. Ranyard: There are observations of Mr. Denning of fixed

radiants not far from the ecliptic. There is one, I think, in Leo. If the meteors from the fixed radiants are moving in space with twenty times the velocity of the November meteors, it would make very little difference whether we met them or whether they caught us up. Every meteor observer knows that the number of meteors seen greatly increases in the morning hours. I do not know whether this is solely due to the increase in the number of planetary meteors met by the earth. If the number of meteors from the fixed radiants (which are generally feeble) increases in the morning hours, it would show that their velocity in space could not be very great compared with the earth's orbital velocity.

The President: I am sure we must thank Mr. Denning, and not only Mr. Denning but also Colonel Tupman, for this interesting discussion.

Mr. Downing read a paper on *Spectroscopic Observations of Nebulæ,* made at Dunecht by Dr. Copeland. It was mentioned in the paper that the comet Wolf was found twelve hours before the telegraphic announcement, which was delayed in transmission.

The thanks of the Society were voted for the paper.

Mr. Chambers: May I ask what has become of the *Monthly Notices* for November?

The President: I believe it is out. The copies were only received from the printer last night, and they were posted early this morning. It is a very thick number, and has given us a great deal of trouble in passing through the press.

The following papers were also announced and taken as read:

The Astronomer Royal: *Observations of Comet c,* 1884 (*Wolf*), *made at the Royal Observatory, Greenwich.*

Mr. M. C. Gogou: *On the Numerical Value of the Co-efficient of Neison's Long Inequality in the Moon's Motion due to the Action of Mars.*

Mr. A. B. Biggs: *Observations of Comets Pons-Brooks and Ross,* 1884.

THE LIVERPOOL ASTRONOMICAL SOCIETY.

The usual monthly meeting was held on December 8th, at the Association Hall, the Rev. T. E. Espin, B.A., President, in the chair. A paper by Professor E. C. Pickering, of Harvard, was read on *The Reduction of Observations for Atmospheric Absorption at Low Altitudes.* The observations in question were made by

Mr. Espin in July last on the Rigi, the object being to test the amount of light that was absorbed by the air when stars were low down on the horizon. Upwards of 50 stars, with a zenith distance of from 80° to 88°, were observed, and compared with others of the same magnitude overhead, and, in some of the lowest, the loss of light was 70 per cent., so that it was very doubtful whether stars below the third magnitude would be visible to the naked eye at less than 2° above the horizon. Further observations of stars near the horizon were greatly needed, and it was to be hoped that other members of the Society would take up the question.

In speaking of the results of the International Meridian Conference, the President said: The Astronomer Royal proposed, after January 1st, to adopt the new method of reckoning time at Greenwich. He thought the change would be of great advantage to astronomers, and he would move that the Liverpool Society follow the same course.

Mr. T. W. Backhouse, F.R.A.S. (Sunderland): Thought the only objection would be the existence of the *Nautical Almanac* for several years yet to come with ephemerides calculated for hours reckoned from noon. This was an inconvenience they must consider, but it seemed to him much better to adopt the recommendation of the Conference, than wait an indefinite time for the change. Some authoritative decision should at once be announced, and very fittingly by the Royal and Liverpool Societies.

Mr. W. S. Franks, F.R.A.S. (Leicester) said: Englishmen ought to be proud that Greenwich was selected to be the timekeeper for the world. He thoroughly agreed with Mr. Backhouse that an authoritative decision should be arrived at, else what was the use of appointing an International Conference of such distinguished men? Twenty-four hour dials had already been introduced to the trade, and the manufacturers would scarcely have ventured on this if they had not felt sure it would be ultimately adopted. The striking of the hours was certainly a knotty point, and it would be rather too bad if, just as one was comfortably settled for the night, all the clocks were to begin striking 24. (Laughter).

Mr. R. C. Johnson, F.R.A.S., thought that, so far as the *Nautical Almanac* was concerned, it would be a very easy matter to subtract 12 hours from any given time, whereas at New York they would have to begin their astronomical day at five hours, and allow one hour for every 15° west. France had not yet given her adhesion to the plan, but, as there was very little difference between the meridians of Greenwich and Paris, it was

very likely they would move their own meridian westward, and so compromise the matter without hurting their pride. After some further discussion the question was put to the meeting and carried unanimously.

Mr. J. Baxendell, jun., communicated a paper on *The Results of a Discussion of* 34 *Maxima and* 18 *Minima of the Variable Star S. Ursæ Majoris,* which he had collected from various sources. These results gave a decided lengthening of the period. From 1855 to 1865 the mean period was 224·071 days; from 1865 to 1875 it was 220·425 days; and from 1875 to 1884, 231·236 days. From 15 of the minima, corresponding to the period 1865-75, he obtained a mean period of 225·334 days, which it would be seen was only ·0091 of a day less than that derived from the corresponding group of maxima.

In some notes contributed by Mr. J. E. Gore, F.R.A.S., on *Some Suspected Variable Stars,* he included 740 B. A. C. Cassiopeiæ; Bradley 396 Cassiopeiæ; 39 Aurigæ, and Birminghams 569 Delphini. The latter had been suspected of variation by D'Arrest, who rated it 8m. on September 10, 1874. His own observations since September last would show that this star was certainly variable and deserving of careful observation.

In a paper *On Red Stars,* Mr. S. M. B. Gemmill said he had found this class of observation much easier than it was generally supposed to be. The diversity of opinion which seemed to exist as to the tints of stars, and especially of red stars, was very striking, and sufficient allowance did not appear to be made for personal and instrumental equation. He thought the designation of *red* was, for the most part, a misnomer, as it was to be questioned whether there was any really red star in the sky. With the exception of certain ruby or crimson stars, he had never seen one which had not a mixture of orange or yellow, and, even in the former class, he thought the tints were not pure.

On the subject of the projection of stars during lunar occultations, the Rev. S. J. Johnson, M.A., F.R.A.S., said the approaching occultation of Aldebaran, visible in this country, led one to look up past cases of the illusion of projection of stars on the moon's limb. The occultation of α^2 Libræ, communicated to the Society last session, was an instance that this phenomenon was not confined to red stars. In the R.A.S. *Monthly Notices* for June, 1828, a list of stars was given that furnished instances of peculiarities in occultations, and it might be as well to note the five different explanations there given.

1. A lively imagination on the part of the observer.

2. A spurious disc given to the moon's image by the instrument of observation.

3. A lunar atmosphere.
4. Irradiation.
5. Different refrangibilities to which the rays from the moon and star are liable, arising from their differences of colour.

He, Mr. Johnson, had noticed the lingering of stars on the lunar disc in the greatest degree during total eclipses, especially in that of 23rd Aug., 1877, when even minute stars appeared several seconds within the reddening limb; though, on the occasion of the last eclipse, the moon presented such a remarkable obscurity of tint that only one instance of this kind appeared, and that not very conspicuously.

On the subject of the November Leonids, 1884, Mr. E. F. Sawyer, of Cambridgeport, U.S.A., said this shower appeared to be very inactive, as during watches of 1h. 40m. on the mornings of Nov. 13, 14, and 15, only two faint meteors were observed which could be attributed to the Leonid radiant. During these periods ten meteors were recorded, which were all quite faint, not exceeding a 3rd mag. star in brightness, whilst their paths were invariably short.

The President said there had been some talk about forming an educational branch for Liverpool. Father Perry had aroused an enthusiasm for astronomy, and he thought it would be a pity if the result of his good work was suffered to die out. It had therefore been proposed to hold elementary classes fortnightly during the winter, and to deliver illustrated lectures on popular subjects.

Mr. Joseph Baxendell thought the number of people who took any interest in astronomy was extremely small, and proportionally less than it was twenty-five years ago.

Mr. W. H. Davies (Secretary) said they had certainly found it very up-hill work in Liverpool, but with beginners a great deal depended upon the way the subject was treated.

Mr. Isaac Roberts, F.G.S., F.R.A.S., suggested that the classes might combine astronomy and navigation, and be in connection with the South Kensington scheme.

Eventually it was decided to hold classes for the study of astronomy only, and Mr. J. P. Weir, 15, Selborne Street, was elected Secretary of the branch. Owing to the pressure of business other papers were taken as read.

Eight gentlemen were elected members of the Society, and twenty-six candidates were proposed.

HINTS TO OBSERVERS OF SATURN.

Fifteen years hence is a long time to look forward to, however short a period it may seem to look back upon; and as it will be all those years before the system of rings round the planet Saturn are again placed in such a favourable position for observing the markings upon their surface as they are at the present opposition, those astronomers possessing telescopes of adequate power cannot do better than take full advantage of their present opportunity.

As Mr. Denning points out in the very interesting article on this planet, on page 265, in Vol. XXII. of this Journal, we have very "little information as to the changes which probably effect the detailed appearance of the planet in different years"; therefore, good useful work may be done by those astronomers possessing efficient instrumental power making *careful* and accurate drawings, and micrometer measures of the different markings they may observe upon the planet's surface; keeping a good look out for anything in the shape of spots as they are likely to give the clue to the nature of the planet's surface. The breadth and colour of all belts should be carefully noted, together with the shape of any spot that may be observed, as they are probably subject to changes of a more sudden nature than the similar markings on Jupiter, though a longer interval intervenes between successive changes on this planet than on the surface of the latter.

With reference to the markings on Saturn, the narrow belt described by Mr. Ranyard, in Vol. XLIV., page 441 of the *Monthly Notices* is worth attention, as it appears to have become more conspicuous than it was at the last opposition.

Another point, too, which deserves much more attention than has hitherto been given to it, is the shadow of the ball on the rings. This shadow is worthy of careful telescopic study, because it does not present at all times those uniform outlines which the laws of perspective teach us should be presented by the shadow of a spheroid projected upon a plane surface; but very frequently this outline is curiously distorted. Observing the planet on the evening of December 5th, 1884, with an 18-in. silver-on-glass reflector and a power of 1,000, the outline of the shadow on the rings was in the form of a truncated cone, as shown in the engraving forming the frontispiece to this volume. This distortion of the shadow on the rings is probably due to the unevenness of the surface of the rings, which is sometimes so considerable as to cause the shadows of the rings on the ball to have a notched appearance, as has been observed by Lassells, Do la Rue, Jacob, and others.

Careful watch should be kept for the occultation of any star by the rings, which it is needless to add is a very rare phenomenon, Dawes being the only astronomer who so far as is known, has had the good fortune to observe a star pass behind the rings. In such a case it should be noted if the star was visible through Cassini's division or not; it probably would be, but it is uncertain as it is possible that Cassini's division is filled with a similar material to that of which the dark ring is composed, and a faint star might be so dimmed as to be invisible. As that careful observer, Jacob, once followed the shadow of the ball across this division, the present time would be very favourable for settling the question, as to whether it is absolutely black or not. While some observers have seen it not quite black, the writer has never been able to observe it of any other colour; but large apertures are requisite to solve the question.

Leyton, Essex. B. J. HOPKINS.

SELENOGRAPHICAL NOTES, JANUARY, 1885.

By Thos. Gwyn Elger, F.R.A.S.

Billy and Hansteen. These closely associated and nearly equal sized ring-plains deserve the attention of the student of the moon's physical features not only on account of the many interesting details presented by the formations themselves but mainly because of the peculiar configuration of the surface for some distance round about them. It generally happens where objects are surrounded by the dark material of the Maria that no visible signs are apparent which lead one to suppose that the latter has been in any way disturbed or has exercised any disturbing influence. In the absence of visible indications of superficial changes, we are impressed with the notion that "the Sea," clearly posterior in date to the object it encircles, has quietly accumulated about the object without appreciably affecting it or its adjuncts. Omitting reference to those formations situated on the shore lines of the Maria, which are usually more or less modified presumably by the action of the material of the latter, there are, however, some others, often far removed from the shore, whose borders and surroundings testify almost as plainly to the destructive effects of this action, of which Billy and Hansteen furnish good examples. If the neighbourhood of these objects is examined under very oblique illumination one cannot fail to notice the

uneven character of the surface, quite irrespective of the many ridges and minor elevations which abound upon it. From the extreme northern limits of Billy to the south-east wall of Hansteen a very wide but well-marked valley extends, the bottom of which is distinguished by a distinct straight dark line probably a cleft. North of Hansteen there are again clear indications of a valley running due north for some distance. Hence these ring-plains are situated in a shallow trough or depression in the surface, and it is rather remarkable that their apparent longer diameters coincide in direction with the course of these valleys,—that of Billy with the first mentioned, and that of Hansteen with the last. This arrangement is very strikingly shown when the two formations are close to the morning terminator and the slope of the ground is consequently seen to the best advantage. Under these conditions the bottom of the valleys is very suggestive of a line of subsidence proceeding from the region between Gassendi and Zupus, under the site of Billy and Hansteen, and extending far away to the north of them. However this may be, the position of the ring-plains is too evidently connected with that of the valleys to be merely a coincidence. Not the least interesting features brought out under a low sun are found in the region close to the north side of Hansteen. Here there certainly seem to be traces of the action of the material of the Mare which appears to have flowed down the gentle declivity representing the western flank of the valley till it was arrested by the opposite side and forced to accumulate like a great mud bank for some miles round the border which it has breached and then invaded the interior, where it may be traced as a large dark oval-shaped mound, occupying a considerable area on the northern quarter of the floor, strongly contrasting in hue with the other portions of it. These accumulations of matter though termed "mounds," for the want of a better word, represent an area of some 200 or 300 square miles. They remind one of the very similar and equally extensive "swellings" seen at sunrise and sunset on the ruined north border of Fracastorius, and are probably due to a like cause. When farther removed from the terminator, these features, with the exception of the gap in north wall of Hansteen, cannot be observed, though others of a different character come to light. Among these is a cleft, forming roughly a tangent to the east wall, running in a northerly direction and terminating at a crater-pit adjoining a craterlet on the south-eastern side of Hansteen, and a well-marked terrace on the inner slope of the south-west wall of this formation. Some little distance west of Hansteen is the conspicuous mountain-mass *a* (*see Neison's Map XII.*) at the south corner of which is a crater. A broad light streak, with parallel sides, representing perhaps a low

ridge, extends from this mountain to the western glacis of the neighbouring ring-plain. A large crater-like object with a dark interior is situated upon it. It is always very indistinctly defined, and resembles an oval scar on the surface of the streak, suggestive of a crater filled up since its formation.

The many bright little mountain-rows and ridges in the country lying north-west of Hansteen, and two or three craters on the south-east side of it are worthy of careful mapping and recording. The question of the relative age of "the Seas" and the smaller detail found upon them is an interesting one, more especially as regards the craterlets, which often, though perfectly similar in size and shape, display every gradation of tint from brilliant white to a hue as dark as the ground which surrounds them. To what cause are these peculiarities due? Are the bright white examples (of which there are two on the south-east of Hansteen) like the ring-plains, older than the Mare, while the darker ones originate in its superficial material, like the mud volcanoes of the Caspian, and are consequently of more recent date? There is some ground for the last supposition in the fact that dark craterlets abound on and about the "mounds" north of Fracastorius, on ridges, and in similar positions. It is also a significant circumstance that the dark floors of many formations, such as Beaumont for instance, are closely pitted with them, which seems as if they must have put in an appearance since these interiors were formed.

The most noteworthy feature in connection with Billy, if we except its abnormally dark and even floor, is the very large and lofty mountain which stands on the slope of the south-western wall. This very prominent and exceptionally situated object is only very faintly indicated in the maps, and is not noticed by Neison. The shadow of the eastern wall of Billy shows that a low pass exists on the northern half of it, and there is a very evident but narrow break in the north wall. There is also a notch on the south wall which indicates the presence of a craterlet or depression.

The valleys associated with Billy and Hansteen are best seen when the longitude of the morning terminator is from 54° to 55°.

Kempston: 19 Dec., 1884.

Errata in No. 264.—Page 305, Radiant No. 4: *For* Dec. +57, *read* +62. Page 306, Radiant No. 21, in column, "Number of Meteors" for 12th day: *For* 64 *read* 57.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

METEORS OF DECEMBER, 8th, 1884.

Sir,—On the evening of December 8, I watched the northern sky for $1\frac{1}{4}$ hours, between 5h. 20m., and 6h. 45m., and saw 14 shooting stars. There was a cloudy interval, accompanied with a light shower of rain, between 5h. 55m. and 6h. 5m. The observations enabled me to determine two radiants, one very sharply defined at $230°+85°$ from 4 short, quick, meteors, all close to it; the other at $214°+62°$, rather ill-defined from 4 slowish meteors. The latter position is suspiciously near the computed radiant point of Pons's comet (1812) but the coincidence is probably accidental, though the fact may be worth reference.

This shower is near α Draconis. I registered a bright 1st mag. meteor conforming to it at 5h. 23m., as it traversed a path of some 15° through the stars of Perseus just under α. The same meteor may quite possibly have been recorded elsewhere, if so it would be important to compare the observations. My path was from $54°+61°$ to $50°+47°$.

At 6h. 45m. a fine Geminid was seen moving very slowly in the southern region of Ursa Major. It was therefore low in the N. by E., and pursued the first part of its horizontal course as a somewhat faint 2nd mag. star, but at about the point $147°+47°$ it suddenly brightened until exceeding Jupiter, and gave a flash which lit up the mist lying over the northern horizon.

Yours faithfully

Bristol: Dec. 15, 1884. W. F. DENNING.

REFRACTOR v. *REFLECTOR.*

Sir,—I think it is to be regretted that the sizes of telescopes are designated by diameters instead of areas, for one can more readily judge of the relative powers when seeing the areas stated rather than the diameters. This is apparent in Mr. Green's letter at p. 308, where he states that he has found a 4-inch

refractor equal to 5-inch reflector, and 5-inch refractor equal to $6\frac{1}{2}$ reflector. Now, I suppose, that the telescopes were all equally good of their kind, and taking the first as a basis it is simply a rule of three question to find the comparative sizes. He states :—

		Refractor.		Reflector.
Diameter	...	4 inches	=	5 inches
Area	...	12·56	=	19·63
And Diameter	...	5 inches	=	$6\frac{1}{2}$ inches
Area	...	19·63	=	33·18

Now, this last is not proportional, for

12·56 : 19·63 : : 19·63 : 30·76 or $6\frac{1}{4}$-inches diameter reflector.

I should like to know if Mr. Green with *the same* eye-piece finds a $6\frac{1}{4}$ diameter reflector equal to his 5-inch refractor. Of course different eye-pieces may give different definition.

R. S. NEWALL.

THE PHENOMENAL SUNSETS.

Mr. C. L. Prince's remarks, taken over into the *Astronomical Register* for August, on the probable cause of the beautiful after glow, besides giving a new explanation to those formerly advanced, are full of interest. Yet, when we consider, even on the most moderate estimates, the total weight, or amount of matter that will be required to be distributed, in the form of such impalpable dust, or crystals, over the whole atmospheric envelope of the earth, and in sufficient quantity to produce so intense a reflection, or refraction (as Mr. Prince asserts), it seems hardly probable to suppose that the Krakatoa outburst could have exerted so great propelling force, within so short a time, or even, supposing the matter, saline, or whatever else, to be so ejected; how did such enormous masses get distributed all over the earth, rather than being precipitated to the surface, on account of the great weight of the whole mass, or volume?

Be the cause what it may, we must hope for a solution of this mystery sooner or later. There seems to me a few decided points of evidence, which render Mr. Prince's theory untenable. Ever since February, I have seen, and so have a great many other independent observers, both here, in Europe, and America, every evening soon after sunset, a bow of light in the eastern sky. This bow or arc being of a dull brick-red colour, proves, I think, that whatever the reflecting medium might be, it is composed of *opaque* matter.

The glow surrounding the sun, as seen in daytime, seems to point to a similar conclusion. Again, Mr. Prince says, that during the winter months of the northern hemisphere the glow was more intense, probably because the cold weather caused a greater amount of crystallization of the saline substances. That this may be probable, I will not dispute; but the reverse has been the case in the southern hemisphere. During the summer months here, the glow was at *its greatest brilliancy*, while as winter advanced it dulled considerably. The variable character it has displayed all through must also be considered.

It has occurred to me, that if we suppose the reflecting media to float in huge semi-detached cloud-like masses, this variability will be explained. If so, we shall be able to study the direction and motions of the upper strata supporting the dust.

JOHN BALLOT.

Rolontein, Transvaal: Sept. 26, 1884.

REVIEWS.

Experimental determination of wave-lengths in the invisible prismatic spectrum. By S. L. Langley. (From the American Journal of Science, vol. xxvii., March, 1884). pp. 20.

The prism employed by Professor Langley, was made by Adam Hilger, of London, and its optical properties are in every way satisfactory. It is of a white flint, which has proved singularly transparent to the longest solar waves. The professor describes an apparatus devised by himself for the measurement of obscure wave-lengths. He could thus test the accuracy of the various formulæ connecting refraction with wave-length, and he finds the formula of Canchy gives grossly erroneous results when extended far behind the limits within which the observations on which it is founded are made. That of Redtenbacher, while satisfying the observations in the visible spectrum, fails when extended to the invisible, and is even less satisfactory than that of Canchy. Briot's formula, though not exact, yet gives results much more trustworthy than the others. Plate V. presents the curves represented by the different formulæ, showing their actual errors by inspection, and that in reality we can scarcely assign any limit to the extent of the infra-red spectrum, and that far from the curve having an asymptote parallel to the axis of X as Canchy's theory requires, one curve (so far as we can follow it) rather tends to ultimately coincide with a straight line cutting the axis at a finite angle, and (if this axis pass through the point $n = 1$) at a great distance from the origin. [n is the index of refraction corresponding to the deviation.]

Every prism gives a different map of the spectrum, and the general conclusions here offered, as to the relation of wave-lengths and indices of refraction, have been drawn from the observations on a single prism, and have not been experimentally verified on others. This is on account of the extremely slow and laborious character of the process used (which has involved some months of labour for this special prism). Though there seems no reason to doubt the generality of the conclusions, it may be hoped that these experiments will be repeated with prisms of other

material, and by other observers, now that the preliminary obstacles have been removed. The concave grating employed is 4 × 5¾ inches, with 18,050 lines, 3,610 to the inch. Radius of curvature 5 feet 4 inches. By this large surface a spectrum is produced sufficiently hot, even in its lower wave-lengths, to affect the bolometer strips after the various reflections and absorptions to which the heat is necessarily subjected in passing through the apparatus. The interest and importance of these investigations are great.

THE PLANETS FOR JANUARY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	19 12 17	S. 20 17½	9″·8	0 27·0
	9th	18 27 39	S. 19 57½	9″·4	23 7·1
	17th	18 24 51	S. 20 54	7″·9	22 32·9
	25th	18 51 7	S. 21 51½	6″·6	22 27·6
Venus ...	1st	16 44 48	S. 21 3	12″·0	21 56·0
	9th	17 27 16	S. 22 18	11″·6	22 6·9
	17th	18 10 22	S. 22 51	11″·4	22 18·4
	25th	18 53 38	S. 22 40	11″·2	22 30·1
Jupiter ...	1st	10 31 20	N. 10 29	3 ″·0	15 43·5
	9th	10 29 51	N. 10 40	39″·8	15 10·6
	17th	10 27 38	N. 10 54½	40″·4	14 37·0
	25th	10 24 48	N. 11 13	41″·0	14 2·7
Saturn ...	1st	5 13 10	N 21 34½	18″·4	10 26·2
	9th	5 10 49	N. 21 33	18″·2	9 52·5
	17th	5 8 49	N. 21 32½	18″·0	9 19·0
	25th	5 7 13	N. 21 32	17″·8	8 46·0
Neptune ...	1st	3 15 14	N. 16 14		8 28·6
	17th	3 14 30	N. 16 12		7 25·0

Mercury rises six minutes before the sun on the 1st, the interval, after a few days, increasing.

Venus rises nearly two hours and a half before the sun at the beginning of the month, the interval decreasing.

Jupiter rises nearly three quarters of an hour after sunset at the beginning of the month, and afterwards earlier each night.

Saturn sets about an hour and a half before sunrise on the 1st, and subsequently earlier each night.

Books received.—Bulletin Astronomique. Par M. F. Tisserand. Tome I. Nov., 1884. Paris: Gauthier Villars. 1884.—Astronomische Nachrichten.—Report of the Superintendent of the Natal Observatory.—Sirius.—Journal of the Liverpool Astronomical Society.—Revue Mensuelle d'Astronomie Populaire. Par M. C. Flammarion.—Ciel et Terre.—Sidereal Messenger.

ASTRONOMICAL OCCURRENCES FOR JANUARY, 1885.

DATE.			Principal Occurrences.	Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Saturn. —
Thur	1	7 57	Near approach of 68 Geminorum (5½) Sidereal Time at Mean Noon 18h. 45m. 12·37s.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	15 16 16 16 17 36 18 55	10 26·2
Fri	2	7 10 7 36 18 57 19 52	Occultation of B.A.C. 2872 (6) Reappearance of ditto Occultation of α Cancri (4) Reappearance of ditto	2nd Ec. D. 1st Ec. D. 1st Oc. R. 2nd Oc. R.	12 26 24 12 32 10 15 49 17 22	10 22·0
Sat	3	10	Inferior conjunction of Mercury and Sun Sun's Meridian Passage 4m. 55·8s. after Mean Noon	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	9 44 10 43 12 4 13 2	10 17·8
Sun	4	14 45 16 0 13	Occultation of 35 Sextantis (6) Reappearance of ditto Conjunction of Moon and Jupiter 4° 2′ N.	2nd Sh. E. 1st Oc. R. 3rd Ec. R. 2nd Tr. E. 3rd Oc. R.	9 58 10 15 11 5 30 11 55 15 0	10 13·5
Mon	5					10 9·3
Tues	6					10 5·1
Wed	7	15 36	☾ Moon's Last Quarter	1st Ec. D.	19 56 58	10 0·9
Thur	8		Saturn's Ring: Major axis=45″·93 Minor axis=20″·68	4th Sh. E. 4th Tr. I. 1st Sh. I. 1st Tr. I. 4th Tr. E.	10 28 14 12 17 9 18 3 18 43	9 56·7
Fri	9			1st Ec. D. 2nd Ec. D. 1st Oc. R.	14 25 16 15 2 46 17 35	9 52·5
Sat	10			1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	11 38 12 30 13 58 14 49	9 48·3
Sun	11			2nd Sh. I. 2nd Tr. I. 3rd Ec. D. 1st Oc. D. 2nd Sh. E. 2nd Tr. E. 3rd Oc. R.	9 35 11 19 11 32 6 12 2 12 31 14 15 18 30	9 44·1
Mon	12			1st Tr. I.	9 16	9 39·9
Tues	13	9	Conjunction of Moon and Venus 4° 8′ S.	2nd Oc. R.	8 55	9 35·7
Wed	14	2	Conjunction of Moon and Mercury 2° 1′ S.			9 31·5
Thur	15	20 36	● New Moon Illuminated portion of disc of Venus=0·895 Illuminated portion of disc of Mars=0·999	1st Sh. I.	19 3	9 27·3
Fri	16	12	Conjunction of Moon and Mars 5° 42′ S.	4th Oc. D. 1st Ec. D. 2nd Ec. D.	16 8 27 6 18 28 17 39 2	9 23·2
Sat	17		Sidereal Time at Mean Noon 19h. 48m. 17·27s.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	13 32 14 16 15 52 16 35	9 19·0

DATE.		Principal Occurrences. h. m.		Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m. Saturn
Sun	18		Sun's Meridian Passage 10m. 48·63s. after Mean Noon	1st Ec. D. 2nd Sh. I. 2nd Tr. I. 1st Oc. R. 2nd Sh. I. 3rd Ec. D. 2nd Tr. E.	10 46 46 12 8 13 36 13 47 15 4 12 29 53 16 32	— 9 14·9
Mon	19			1st Tr. I. 1st Sh. E. 1st Tr. E.	8 42 10 20 11 2	9 10·7
Tues	20			2nd Oc. R.	11 15	9 6·6
Wed	21	5 39 6 52	Occultation of B.A.C. 57 (6½) Reappearance of ditto			9 2·4
Thur	22	5 33 6 47	Occultation of *e* Piscium (5½) Reappearance of ditto	3rd Sh. E. 3rd Tr. E.	9 1 11 33	8 58·3
Fri	23	13 26 22	☽ Moon's First Quarter Conjunction of Venus and Mercury 1° 6′ N.	1st Ec. D.	18 11 45	8 54·2
Sat	24			1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	15 26 16 1 17 46 18 21	8 50·1
Sun	25			4th Tr. E. 1st Ec. D. 2nd Sh. E. 1st Oc. R. 2nd Tr. I. 2nd Sh. E. 2nd Tr. E.	9 40 12 40 5 14 41 15 31 15 52 17 38 18 47	8 46·0
Mon	26	7 13 7 49 14	Occultation of B.A.C. 1526 (6) Reappearance of ditto Conjunction of Moon and Saturn 3° 27′ N.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	9 54 10 28 12 14 12 47	8 41·9
Tues	27	8 38 9 30	Occultation of B.A.C. 1930 (6½) Reappearance of ditto	2nd Ec. D. 1st Oc. R. 2nd Oc. R.	9 33 1 9 58 13 33	8 37·8
Wed	28	14 23 15 21	Occultation of λ Geminorum (3½) Reappearance of ditto Saturn's Ring: Major axis=44″·75 Minor axis=20″·19	1st Tr. E.	7 13	8 33·7
Thur	29			2nd Tr. E. 3rd Sh. I. 3rd Tr. I. 3rd Sh. E. 3rd Tr. E.	7 54 9 19 11 19 12 59 14 54	8 29·6
Fri	30	4 19 8 54 9 59	○ Full Moon Occultation of B.A.C. 3122 (6½) Reappearance of ditto			8 25·5
Sat	31	19 6 35 16 16 17 20 17 50	Conjunction of Moon and Jupiter 4° 9′ N. Occultation reappearance of π Leonis (5) Occultation of B.A.C. 3529 (6) Reappearance of ditto Near approach of 43 Leonis (6)	1st Sh. I. 1st Tr. I.	17 20 17 46	8 21·5

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator, E., Evening Terminator.

1885.	° ′		° ′		° ′
Jan. 1	+81 56 E.	Jan. 11	−39 38 E.	Jan. 21	+18 46 M.
2	69 46	12	51 48	22	6 37
3	57 36	13	63 57	23	− 5 33
4	45 27	14	76 7	24	17 42
5	33 18	15	88 16	25	29 52
6	21 9	16	+79 34 M.	26	42 2
7	8 59	17	67 24	27	54 11
8	− 3 11	18	55 15	28	66 21
9	15 20	19	43 5	29	78 30
10	27 30	20	30 56	30	+89 20 E.
				31	77 10

Moon farthest from the Earth, Jan. 12, 21h.
(in new Greenwich time, „ 13, 9h.)
„ nearest Earth „ 28, 14h.
(in new Greenwich time, „ 29, 2h.)

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To April, 1884.

Elger, T. G.

To Dec., 1884.

Backhouse, T. W.
Dansken, J.
Joynson, J.
Remington, Major

To Dec., 1885.

Böe, A. de
Clermont, Lord
Ellis, W. M.
Rogerson, G. R.
Rylands, G.
Walker, G. J.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

***Post Office Orders* for *the Editor* are to be made payable to JOHN C. JACKSON, Chief Office, London.**

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 266. FEBRUARY. 1885.

ROYAL ASTRONOMICAL SOCIETY.

Session 1884—85.

The January meeting was held at the Society's Rooms, at Burlington House, on Friday, Jan. 9th, 1885.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—Mr. E. B. Knobel, and Col. Tupman.

The minutes of the preceding meeting were read and confirmed. 53 presents were announced, and the thanks of the Society were voted to the respective donors.

The following gentlemen were balloted for and duly elected Fellows of the Society:

Lieut. W. St. L. Chase, Quetta, Baluchistan.
Latimer Clark, 6, Westminster Chambers, Victoria Street.
Walter Goodacre, 6, Gurney Villas, Clova Road, Forest Gate, E.
Geo. Hamilton, F.C.S., Westbourne, Penkett Road, Liscard, Cheshire.
Rev. Andrew Henderson, Paisley.
William John Ibbetson, B.A., Clare College, Cambridge.
Rev. Geo. W. James, F.R.H.S., Tuscarora, Nevada, U.S.A.
Asutosh Mukhopadyay, 68, Russa Paglah Road, Bhowanipore, Calcutta.
Prof. Kavasjee D. Nacgamvala, M.A., F.C.S., Elphinstone College, Bombay.
Thos. Cunningham Porter, B.A., Newtown House, Redland, Bristol.

Capt. B. Thomson, R.N.R., Ghyllbank, St. Helen's, Lancashire.

H. H. Turner, B.A., D.Sc., Royal Observatory, Greenwich.

James Wigglesworth, New Park House, Falsgrave, Scarborough.

The President said: I have been requested by the Council to inform the meeting that to-day they have awarded the gold medal of the Society to Dr. Huggins, for his observations of the motions of stars in the line of sight, and for his photographs of the spectra of stars; and I am certain you will all coincide with the Council in their decision. (Applause).

Mr. Knobel regretted that there had been great delay in the publication of the January number of the *Monthly Notices*, owing to various causes. The number will contain twelve woodcuts and one plate, and the papers, which are very long, have all had to be corrected by the Assistant Secretary. In consequence of a mistake having been made in some of the woodcuts, it was necessary to recut them.

Mr. Spitta read a note on an observation of Saturn, made on November the 23rd, with a reflecting telescope of 10-inches aperture. His attention was directed, during indirect vision, to a point of illumination at the edge of the crape ring. Applying the occulting eye-piece which he described at the December meeting, he found the brightness was unmistakable, but it lasted only a short time. The brightness was ill defined. He could not say how long it lasted, but it was probably for ten minutes with an error, say, of 1½-minutes. He could not state whether it was due to a star occulted by the planet, but his friend, Mr. Coleman, had found four stars that might have been occulted by the planet, and one of them was of the 11th magnitude.

Mr. Coleman: I may say I made my observations with the best means at my disposal, but I cannot say that they were very accurate.

Mr. Ranyard: It could not have been one of the satellites, for in the present position of the planet the ring is inclined at an angle of about 26° to the line of sight. The satellites move in orbits which do not differ greatly from the plane of the ring, so that they would either pass above or below the ring.

Mr. Spitta: It might be due to a satellite whose orbit lay between the rings and the ball: its nodes might retrograde to such an extent that you could possibly see it just on the swing, turning the corner, as it were.

Mr. Stone read a paper on the statements of Mr. Gill as to the effects of *Serious Systematic Screw Errors in North*

Polar Distances in the Cape Catalogue of 1880. He said: It was much more easy to assert that there were serious errors due to wearing of the screws between 1856 and 1879 than to prove it. That two surfaces which rub must to some extent wear was perfectly true, but that the relative wear over the five or six threads used was sufficient to lead to serious errors could only be settled by an appeal to facts. Fortunately, whilst at the Cape, he directly examined this question with regard to the screws as a whole. The results of observations of the nadir points at 0^r and 2^r in 1875, as printed in the Cape Catalogue in 1875, showed no serious difference, but in 1877 he examined the threads again, and showed that so long as the observations were restricted to the five or six threads of the screws in constant use the errors due to the screws were confined within very narrow limits, but the employment of one or two threads outside the necessary range soon brought relative errors into existence. The rapid increase of errors near the end of the series showed that it is utterly useless to compare errors of screws at different times unless it were certain that no change had taken place in the adjustment of the index from which the revolutions were taken. Adjustments were as a matter of fact made from time to time, and that was probably the explanation of Mr. Gill's remarks. It is, however, possible to apply a severe test to the Cape observations of 1880, with respect to the existence of serious errors due to screw wear. The Cape transit-cone was brought into regular use in 1856, and during the years 1856 to 1860 a considerable number of the stars which have been observed with regularity at Greenwich since Bradley's time were observed at the Cape. As the screws at this time were new we can assume that the screw wear was insignificant. These N.P.D. are contained in the Cape Catalogue for 1860. Proper motions, deduced from the Greenwich observations, and therefore quite independent of the Cape results, of some accuracy can be found for these stars, and we can bring up the N.P.D. for the epoch 1860 to 1880, and compare the Cape results made 1856-1860 with those made 1870-1879. The screws are asserted by Mr. Gill to be so seriously worn as to lead to large systematic errors in the latter results. I have made these comparisons and the results are given in the present paper. The probable error found directly from these comparisons is certainly not greater than that due to the mere errors of observation without assuming any increase of errors due to screw wear. A comparison of 92 stars gives only a relative probable error between the Cape results 1860 and 1880 of $0''\cdot28$, whilst if there was a systematic error due to screw wear of $0''\cdot5$, the probable

error would certainly be as great as $0''\cdot60$. It is hardly possible to apply a more severe test, but the curve of nadir discordancies over all the threads of the screws which could be used, which I made in 1877, and which is contained in Mr. Gill's paper, will show at once that no large systematic error could possibly accrue to the mean in these observations from such screw wear as existed in the Cape screws when the observations were restricted to the central threads. With respect also to the statements respecting the right ascensions of the 1880 Catalogue, as affected with systematic errors, due to a supposed lateral refraction or variable plane of collimation which Mr. Gill has made in the introduction to the Cape Catalogue 1850, and simply based upon the differences between these two Catalogues brought up to a common epoch, I must remark that no materials at present exist for the bringing up of the southern stars over a period of 30 years with sufficient accuracy to justify any attempts to bear discussion of systematic instrumental errors upon such differences. The method is a most unsatisfactory one when the observations extend over 100 years, as in the north, but is quite illogical with respect to southern work. Such differences as 0·06s. which Mr. Gill has found for the mean discordance between these Catalogues is not greater than the possible errors of our determination of the constant of precision; and certainly not greater than the difference between systems of right ascensions which different astronomers have adopted for clock errors. To test, however, clearly whether there is any plausibility in assuming such differences to be due to a variable plane of collimation, I have compared the Melbourne Observations 1875, with the Cape Catalogue results for 1880. As the observations contained in the Cape Catalogue were made 1870-1879, there will be, in this comparison, but little error due to adopted motions or their neglect. In making this comparison, as the clock errors are all based on the Greenwich standard proper list, I do not use the stars of less N.P.D. than 124°, but between 124° and 170°. I have found 46 stars common to the two Catalogues, and the mean excess of the Cape right ascension is only 0·02s., and the individual results all closely agree. Unless, therefore, the variation in the plane of collimation is the same at Melbourne and the Cape there is certainly no sensible variation at either station. Of course, if the Greenwich right ascensions are two small by 0·06s., then the Cape right ascensions are too small by the same quantity, if well made and accurately reduced.

The President: I am glad that Mr. Stone has thought fit to come before us with this explanation. The moment I received the Cape Catalogue of 1880 I examined the positions of several

stars, and found them to agree with those of other catalogues, and I am surprised to find that corrections are necessary to be applied for systematic errors depending on the screws. I have no doubt the wearing of the screws has some effect, and I do not think Mr. Stone will say it has not; but when it is said that it has any great effect I cannot agree with that. I am sorry I have not had an opportunity of studying Mr. Gill's paper, but Mr. Stone's explanation is a very clear one, and one which shows that the supposed effects of systematic errors are larger than they really are. Looking through Mr. Stone's paper this afternoon, I was struck very much with the smallness of the errors he pointed out, and I think he has answered Mr. Gill fairly on the question. Great credit redounds to Mr. Stone for his Cape Catalogue, and I have no doubt it will come out unscathed from any reflections that have been made on it.

Mr. Knobel read a note *On the description of two stars (α Scorpii and α Orionis) in Ptolemy's Catalogue.* In the Liechtenstein Almagest, published at Venice in 1515, the descriptions of the eighth star in Scorpio and the second star in Orion contain the expression "tendens ad rapinam," the meaning of which had hitherto baffled all students.

As this edition of the Almagest was translated from the Arabic by Gerard of Cremona, Mr. Knobel thought a solution of the difficulty might possibly be found by consulting other Arabic MSS. The paper contained the results of the investigation of four Arabic MSS., two Almagests at the British Museum and Bodleian libraries, and two copies of Al Sūfi at the British Museum and India Office. The corresponding word for "rapinam" in three of these MSS. and in the MS. of Al Sūfi, examined by Professor Schjellerup, was given as El Husi, El Chusi, El Hud, El Harad or El Harah. Their general resemblance suggested copyists' errors. It was clear that all these words were intended as translations of the Greek word υποκιρρος, which Ptolemy uses to describe the reddish color of the star. No meaning could be found for the first three Arabic words. Dr. Redhouse had suggested that the true word was El Harad, which meant "sallowness," or "pallor." This opinion corresponds fairly with the expression "cerea," used in another passage of the Liechtenstein Almagest as a translation of υποκιρρος, the exact Arabic word for which, "shamaie," "waxlike," was found in the British Museum Almagest. Mr. Knobel thought it very probable that Gerard of Cremona had found the same untranslatable words which he had shown to exist in the MSS. quoted, and that he had in this difficulty given a meaning having the astrological significance of these particular stars. He also pointed

out some curious mistakes of translation in the MSS. of the Almagest at the British Museum.

The President: I am sure we are much obliged to Mr. Knobel for this communication. When we know that there are very old observations in the archives of the various libraries on the Continent and in England, anyone who will take the trouble to investigate them, so as to elucidate some of the questions of the early history of astronomy is doing good service. I have been informed that it is a very difficult thing indeed to decipher some of these old manuscripts. Anyone attempting to decipher the old writing of 600 or 700 years ago finds many difficulties, and one might fancy it was a great waste of time. I know that some persons will take a whole day to decipher even one line of such writing; but if anyone will have the patience which Mr. Knobel has shown in examining old astronomical works, though he may spend a good deal of time, he will come to some useful conclusion at last.

Colonel Tupman read a paper by Mr. Downing *On the periodic time of α Centauri.* The paper was in reference to one which appeared in the *Monthly Notices*, by Mr. Powell, in which it was stated that the orbital period of revolution was not less than 86 or 87 years; but Mr. Downing, after comparing several observations, believed that the period of 76 years was more in agreement with the observations made before Herschel's time, and recently by Mr. Tebbutt and others.

Mr. Stone: Some of the southern observers have been using α Centauri for their instrumental corrections, but such a use of it should not be made unless the two stars are taken together. One of the surveyors in New Zealand has been using α Centauri, and has found the catalogue place considerably out; and it is perfectly true that it would be so. In the Cape catalogue of 1880 I adopted the mean proper motion derived from the motion of the two companions. If any one will use α Centauri for determining instrumental errors, I should say they had better not do so unless they observe both components.

Mr. Knobel produced a new series of graphical representations of orbits prepared by Mr. Marth, who had also sent a brief written explanation, stating that he had adopted a plainer method of showing the orbits than in the diagrams submitted to the preceding meeting. A circular orbit in the plane of the ecliptic is represented by a dot; an eccentric orbit in the same plane is represented by a line; an eccentric orbit inclined to the ecliptic is a curved arc. The paper furnishes the data for determining the orbits of 150 minor planets. Another paper would furnish the data for the periodical and other comets. These

graphical representations were intended to serve in the investigation of close proximities.

Mr. Knobel read a note by Mr. W. S. Franks on Professor Pritchard's recent papers on a comparison of the light transmitted by a reflecting and refracting telescope. Professor Pritchard, he said, seemed to infer that nothing had been done towards a careful comparison between reflectors and refractors since Dr. Robinson's observations, but contributions had appeared on the subject in the *Astronomical Register* for 1867 and 1872, in another magazine for 1876, and in the *English Mechanic*, by himself, in 1879; and also in the 20th vol. of the *English Mechanic*. Dr. Robinson's observations did not go far enough, because he ignored the relative focal lengths of the telescopes used. In the case of the instruments used by Professor Pritchard, the focal length of the object-glass was 15·9 feet, while the focal length of the two reflectors was about 10 feet. But for useful and correct comparisons the foci should be identical, and the angle of convergence of the rays should be similar; the same eye-piece should be alternately used on each instrument, and the instruments should be placed side by side under precisely similar atmospheric conditions, and should be directed to the same object; also the observer should in each case be the same. These conditions had not been fulfilled in Professor Pritchard's experiment; two different observers seem to have been employed, and two different wedges and eye-pieces to have been used. It was scarcely possible to have two wedges or two eye-pieces precisely alike. If a London smoke-wedge were used the star should be white; but some of the stars on Professor Pritchard's list were coloured. He could not agree with Professor Pritchard that it had long been known that the reflective power of silver exceeded that of speculum-metal, for he believed it was not the fact. He doubted also the statement made by Mr. De la Rue that speculum-metal reflected the actinic rays more than silver on glass, because if that were so, how was it that a speculum-metal reflector imparted a distinctly red tone to the objects observed?

Mr. Common: I am very glad of this opportunity of making a few remarks. When Professor Pritchard read his paper, I took exception to the way he made his observations. I have since been considering the question, and am led to feel that I was mistaken. Mr. Franks has gone on the same lines that I went on rather hurriedly. I do not think the angle of incidence of the cone of rays can make much difference. I believe the experiments which Professor Pritchard made were perfectly correct as regards estimating the actual light transmitted, whether by the reflector or refractor. As to the state of the reflectors that he then employed,

I have nothing to say. Whether you place the medium by which you extinguish the light in the focus or out of the focus, matters not. The same effect will be produced by a piece of glass with the same light-extinguishing properties, whether the rays have come to a focus or not. Whether a whole mass of glass is placed before the object glass or before the reflector, matters not. If the glass has equal extinguishing powers, it will only have the same effect. The angle of incidence has nothing to do with it. With regard to the paper of Mr. Franks, as to what he said about the lenses of the various eye-pieces, this does not affect the question. The apparatus Professor Pritchard used was a photometer devised by himself, and executed by Mr. Hilger, and that was applied to both telescopes. The fact of the two wedges being used does not alter the question, because the relative effect of the wedges in extinguishing light was taken into account. After considering the position of the whole thing, the only conclusion I could come to as regards the result that Professor Pritchard has obtained was this, that the surface of silver he experimented upon, and used for the refractor, had not such a surface as it ought to have had. He did not state what the flat was made of, whether it was a silver film or of speculum metal, but I may state shortly my opinion as to the reflective powers of silver on glass newly polished, and the best object glass; it is this—and in that opinion I am borne out by experiments which have been undertaken on the continent, and which were described to me by Dr. Schroeder, who is now the optical head of the firm of Ross & Co., opticians, London. That gentleman made a series of experiments, in conjunction with Professor Zolner, using a Zolner photometer, and the conclusion they came to, from their exhaustive series of experiments, was that the reflective power of newly deposited and well polished silver was greater than the transmitting power of any object glass, but that the surface of the silver deteriorated very rapidly indeed after a very short exposure to the atmosphere. The reflecting power of the silver before it is diminished, approaches very closely to the effective power of the ordinary object glass. After that, its power is lowered very slowly, and it soon gets into a permanent condition of dullness, and remains so for a considerable length of time. But with regard to the speculum metal, they found that its reflective powers were pretty constant. If you take silver on glass at the very first, you have a reflective power which equals, if it does not surpass, the best object glass existing. After a short time its reflective power diminishes very quickly, and approaches that of speculum metal, and the two then go on together. The speculum metal is always below

the object glass, but the silver on glass after a time remains constant for a pretty long time. The general idea with regard to the object glass is that 90 per cent. of the light is transmitted—but when you consider that, from a glass mirror, without any silver on its face, you can get 5 per cent. of light, it follows that from the four surfaces of the two lenses of an achromatic object glass 20 per cent. of the light must be reflected back into the air. In the case of an object glass made 20 years ago, the loss of light would amount to 30 or 40 per cent. and the very worst speculum metal reflector would be equal to some of the worst specimens of object glasses. But as to settling these questions of the comparative merits of silver on glass, as against the transmitting powers of object glasses, you cannot possibly do it; generally you can only estimate the reflective powers of the particular speculum of silver on glass, silvered at a certain date; the question cannot be possibly settled generally. The conclusion I have come to in my own mind is that we cannot neglect the experiments of Professor Pritchard, because he has evidently made them with great care. I am rather a believer in the wedge, having tried it several times. In dealing with the determination of the actual effective powers of these telescopes as condensers of light that makes itself apparent to the eye, the size of the beam passing through the wedge may be neglected. What the effect is on the retina is the only test. We are not dealing at present with the effect of the light that reaches the focus for photographic purposes. Prof. Pritchard acknowledged that the silver on glass mirror he used had been silvered three years before. If these experiments had been made with a new silvered surface, and with a silver flat that was new, he would have found, that instead of the proportions coming out as he got them, that the silver on glass reflector would perhaps have been as effective as the refractor. I cannot agree with the remarks of Mr. Franks in his paper, but as to the way in which Prof. Pritchard made his trials I believe they were perfectly correct.

Mr. Ranyard: Mr. Common has omitted one element with regard to the silver film made use of which is very important. Mr. Calver has, since the last meeting, pointed out that a thin silver film allows a great deal of light to pass through it. Even with a thick film the light of the sun can be seen through the mirror, and with a thinly deposited film, or a film that has become thin by polishing, a great deal of light must be transmitted, and, of course, never reaches the eye. I do not agree with Mr. Common that the reflection from each of the four surfaces of the lenses of an achromatic object-glass can be taken as reducing the

light by five per cent. The space between the two middle surfaces is usually filled with Canada balsam, and since the amount of light reflected at a surface is proportional to the differences of the reflective indices, the amount of light reflected must be small, for the refractive index of Canada balsam differs very slightly from the index of crown glass. But there will be the five per cent. loss at the front surface, and five per cent. loss at the back surface of the object-glass as well as a great deal of absorption by the glass, when the object-glass is large and thick.

Mr. Knobel: I understand Mr. Common to say the mirror used by Prof. Pritchard had been silvered two or three years. I have received a letter from Prof. Pritchard on the subject, in which he says the mirror was silvered originally 5½ years ago by Mr. With, and that since the light was measured it has been resilvered, and when the silver film was quite fresh the light-collecting power of the telescope was again measured, and the difference amounts to less than five per cent.

Mr. Ranyard: Perhaps the film on the second occasion was very thin.

Mr. Knobel: It may be so, but we must presume that a proper film was deposited similar to the first one.

Mr. Ranyard: Everybody who has silvered a mirror knows that a great deal of its reflective power depends upon its polish.

Mr. Spitta: As regards Canada balsam, you would not have it in such a telescope as Professor Pritchard used.

Mr. Ranyard: But they usually use oil when there is no Canada balsam.

Mr. Spitta: I think not with Grubb's instruments.

Mr. Rand Capron: I think in experiments of this kind it would be well to substitute a prism for the flat as a second reflector. The reflective power of the prism could be easily ascertained, and it would be less variable as compared with the flat. I speak from experience, having used a totally reflecting prism as a substitute for a flat with my 8½ reflector, and I found an increase of light, and a great deal more certainty in its use.

Captain Abney: With reference to Mr. Ranyard's remark about the thinness of the film I may say that it would affect the violet rays principally. It is the violet rays that are transmitted; they are not the most luminous part of the spectrum, and therefore the thickness or thinness of the film practically does not make very much difference. In regard to Mr. De La Rue's remark at the last meeting as to the actinic rays being more reflected by the speculum metal than by silver, he was perfectly correct. Perhaps I may state that for spectroscopic work I have done away entirely with lenses, and I have been using reflectors, both

for the collimator and the telescope of my spectroscope. By that means I get a brighter image than with the collimator and telescope lenses; I think that bears out the remarks Mr. Common made just now.

The following papers were laid on the table and taken as read:—

J. Gledhill: *Occultations of Stars by the Moon and Phenomena of the Satellites of Jupiter and Saturn observed at Mr. Edward Crossley's Observatory, Bermerside, Halifax, in the year* 1885.

E. J. Spitta: *Observations of Stars occulted by the Moon during the eclipse of Oct.* 4, 1884.

E. J. Stone: *The observations of the Moon made at the Radcliffe Observatory, during the year* 1884, *and a comparison of the results with the tabular places from Hansen's Lunar Tables.*

Communicated by Astronomer Royal: *Observations of occultations of stars by the Moon, and of phenomena of Jupiter's Satellites made at the Royal Observatory, Greenwich, in the year* 1884.

Rev. Ad. Müller: *Observations of Comet c.* 1884, (*Wolf*) *made at Stonyhurst College Observatory, with the* 8-*inch telescope, and ring micrometer.*

THE LIVERPOOL ASTRONOMICAL SOCIETY.

The fourth meeting of the session was held on January 12th, the President occupying the chair. Mr. W. H. Gage, F.R.A.S., submitted a list of coloured stars in the constellation of Leo. He had also prepared a list of the colours as they had appeared during bright moonlight. Exception had been taken to some remarks he had made in his last paper, and he thought it would render his meaning clearer by explaining that his object had been to describe these colours with as little ambiguity as possible, because, if any law existed as to the distribution of colour, it would be likely to escape detection by employing a multiplicity of terms. It was often difficult to describe the exact tint of a star, and he would, for example, class a star as "white, lilac tinge," when the star on the whole was white, with an occasional flash of lilac.

Mr. W. S. Franks, F.R.A.S., said: It had been stated in a paper read by Mr. Gemmill before the last meeting that it was doubtful if there was any really *red* star in the sky; but, whilst agreeing that many reputed red stars were merely orange, he would contend for the existence of stars which were indisputably red, and only the other evening he came across B 521 Cygni, which was like a drop of blood on the black sky. In colour

observations so much depended on aperture, and it was scarcely necessary to point out that a small refractor was wholly inadequate to deal with them. His own experience confirmed that of Huggins', who found colour imperceptible with either too much or too little light. Evidently, then, in any comparisons of the results of different observers, due regard must be paid to the apertures used by them. Variations in colour, also, would often lead to misconception; he had, for example, observed Mira Ceti some time ago, and entered it as yellow, but during last month he saw it decidedly red.

The President said they had fortunately a guide to colour in photography. He had often noticed that certain stars were either below their normal magnitude or were missing altogether from the plate; on looking up these stars with a telescope he invariably found they were red.

Mr. J. E. Gore, F.R.A.S., would like to point out that Mira Ceti, alluded to by Mr. Franks, was now rapidly rising to a maximum which promised to be exceptionally bright. It was already 4½ magnitude, and very favourably situated for observation, as it was on the meridian early in the evening.

The Rev. S. J. Johnson, M.A., F.R.A.S., called attention to the approaching occultation of Aldebaran. The first of this group of occultations happened on the 30th of December last, but its limiting parallel was 55° N., so he supposed it was for this reason omitted from the list of occultations in the *Nautical Almanac.* It would, however, be visible in Scotland, and he hoped Scotch members would send some particulars of their observations.

Mr. E. F. Sawyer, Cambridgeport, Mass., was pleased to see that Mr. J. Baxendell, jun.'s observations of R. Ursæ Majoris, as reported in the journal for November, fully agreed with his own, which had extended from August 11 to November 6th, and were 32 in number. He had noticed that the brightness increased from 8·2 on the 11th of August to 7·1 on the 29th. The increase, therefore, was very rapid, and about half a magnitude more this year than last.

Mr. Thos. G. Elger, F.R.A.S., read a paper illustrative of a drawing of Aristarchus and the W. wall of Herodotus. The observation was made on the 8th of March with powers of 350-400. He also described the remarkable clefts or valleys which lie between the two formations. The meeting adjourned at 21h. 30m.

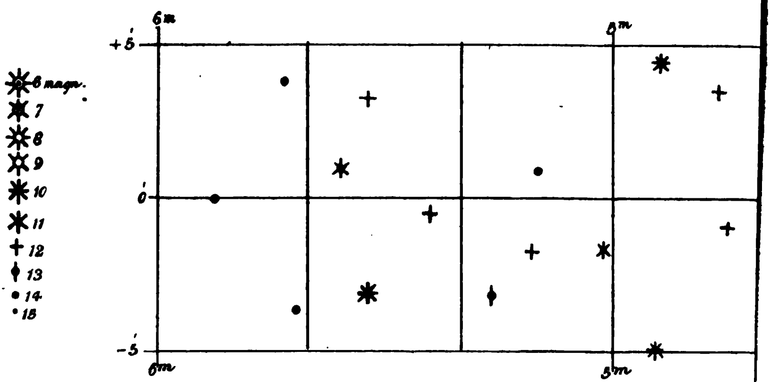
SELENOGRAPHICAL NOTES, FEBRUARY, 1885.

By Thos. Gwyn Elger, F.R.A.S.

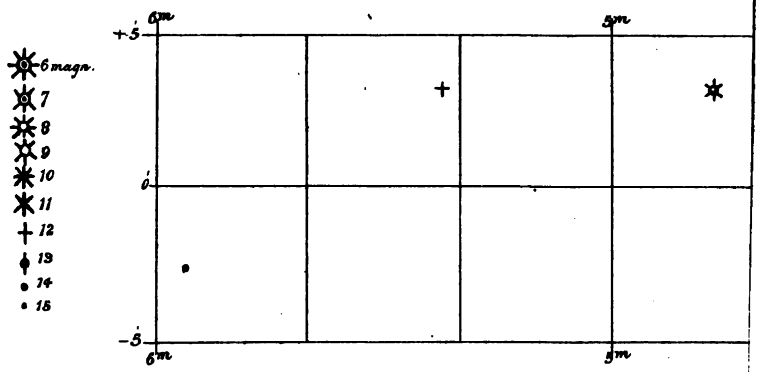
Cardanus and Kraft. On 30th December, 1884, at 20h., the western borders of these formations were on the morning terminator, a broken line of light faintly indicating the loftier portions of their eastern walls, while a low ridge just emerging into sunlight represented a part of the very irregular ring-plain, Olbers *c*. The curious cleft, discovered by Schmidt in May 1872, uniting Cardanus and Kraft, was a most conspicuous feature. It traverses the north-western border of the former and, after pursuing a nearly straight course for the greater part of its length, suddenly turns towards the west as it approaches Kraft, and apparently terminates in a wide chasm on the south-east side of the crater *c*, situated on the south-west glacis of the latter ring-plain. The sudden bend is evidently occasioned by the wall of Kraft, round the foot of which it appears to curve. A noteworthy peculiarity of this fine cleft under morning illumination is that it has a light border on *both* sides, a circumstance which perhaps accounts for it not being recognized as a cleft by Mädler and others, who seem to have regarded it as the shadow of a ridge. It is probably in part a crater-cleft, and the remainder being considerably above the general level of the neighbouring surface, its western light border is explained by the presence of a bank sloping up to the cleft from the mare below towards the sunlight. There are not wanting other instances of clefts having light borders on both sides, notably that running between the craters *a* and *c* on the floor of Schickard, to be accounted for possibly in a like manner, or on the supposition that they are crater-clefts. On the same evening another cleft was remarked south of Cardanus, cutting through the ridge on the terminator pertaining to Olbers *c*, and following a very winding course towards the south-west, or in the direction of a little crater marked *c* in Neison's Map XI., some distance to the west of Cardanus, though it appeared to die out before reaching it. The greater portion of this object was very distinct with a power of 400 on my 8½-in. Calver reflector, especially the eastern end in the neighbourhood of Olbers *c*. On the western side of Cardanus, a little beyond the foot of the wall, there appears to be a mound or low mountain forming the southern termination of one of the branches of the long ridge which runs in a southerly direction from Seleucus: the other, or eastern branch, joins the northern wall of Cardanus, immediately west of the cleft which connects this formation with Kraft. A

very cursory examination of this region is sufficient to show that both Mädler's and Neison's maps give very imperfect representations of it, and are far inferior to Schmidt's chart. A great deal too much is made of the very anomalous formation Olbers *c*, which is little more than a curvilinear arrangement of ridges, while Neison's drawing of Kraft altogether fails to give one a just idea of its importance as a distinct ring-plain surrounded by a wall of considerable altitude. On the following evening, 31st December, 20h. to 21h., the cleft connecting Cardanus and Kraft was only just traceable as such from the north border of the former to about half-way between it and the latter. Except at moments of best definition it resembled a light spur or streak proceeding from the eastern branch of the great Seleucus ridge where it joins the north wall of Cardanus. The cleft running from the south-east wall of Cardanus towards Olbers *c* was also well seen. About the middle of its course it passes between two small craterlets west of Vasco de Gama A. No traces of the winding cleft seen on the previous evening west of this region were observed, though it is probable from its position that it joins the cleft running south of Cardanus at or near these craterlets. Schmidt shows a mountain within Cardanus which was searched for without success, though the low narrow ridge on the south-west side of the interior of the companion formation was easily made out. Mädler and Neison both ignore the existence of a large and very distinct crater on the south-west side of Cardanus, which, under a high sun, figures as an oval white spot.

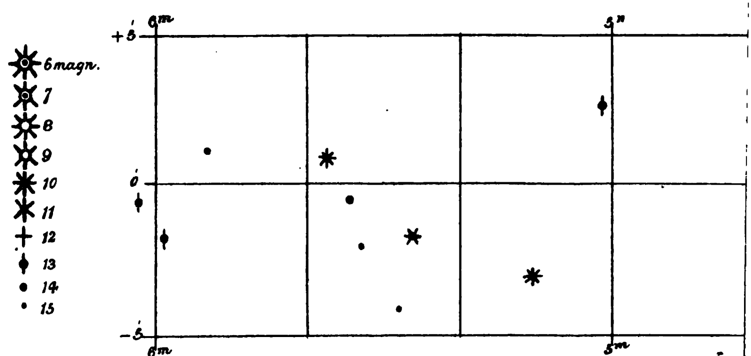
The great Sirsalis Cleft. The southern portion of this cleft was examined between 19h. and 19h. 30m. on 30th December, 1884, with a power of 400, definition being moderately good. No indications of the extension of it through Byrgius were visible. After cutting through the walls and interior of De Vico *a* it becomes broader and more irregular, till it is ultimately confounded with a ridge which follows the bold curve of the cleft to a point north of Byrgius. Before encountering the ridge it sends out a branch to the mountain range, which extends between Byrgius and Cruger *a*. On the rough plain beyond this range its direction is prolonged by a magnificent cleft running for miles in a north-easterly course towards the limb through an unnamed region. Adjoining the south-western side of De Vico *a* are two confluent craters, round the western border of which winds a cleft that ultimately joins the Sirsalis cleft just before the latter reaches the border of De Vico *a*. There is a long ridge forming a tangent to the eastern wall of this ring-plain, and, at the place of junction, adding considerably to its breadth. The great



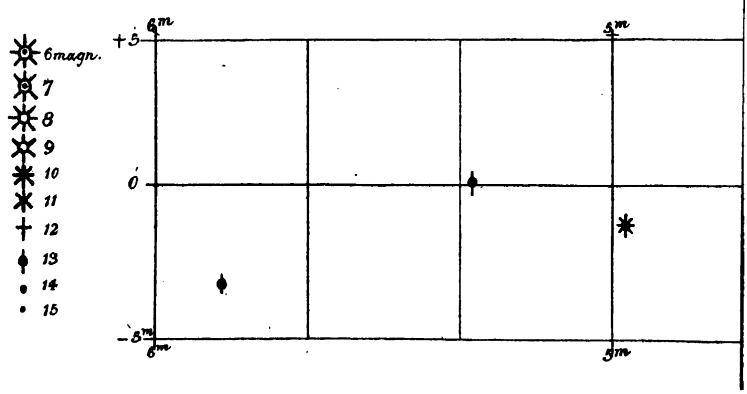
Region following γ Pegais.



Region following ε Orionis.



Region following η Virginis.



Region following η Serpentis.

cleft cuts through the wall at this point in a more attenuated form than elsewhere, a short spur from the ridge bounding it on the north after it has left De Vico *a*. Drawings of this cleft on a much larger scale than any that have hitherto appeared are needed to show its details and those of its surroundings satisfactorily.

Kempston, Beds: January 19, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

FAINT STARS FOR STANDARDS OF STELLAR MAGNITUDE.

The charts accompanying this circular represent four out of twenty-four regions, from which it is proposed to select certain stars for standards of the magnitude of faint stars. Additional information with regard to the plan is given in the printed reports sent herewith. The regions represented in these four charts are those from 2m to 6m following the bright stars of γ Pegasi, ϵ Orionis, η Virginis, and η Serpentis. Each region extends 5′ north and 5′ south of the declination of the corresponding bright star.

It is desirable that these charts should be made as complete as possible, and it is hoped that astronomers having the use of powerful telescopes will assist in accomplishing this object. They will confer a favour upon the Observatory of Harvard College by comparing these charts with the regions which they represent, and marking upon them the places of any additional stars which may be visible. Some indications of the comparative brightness of these stars would also be desirable.

Astronomers who may be disposed to take part in this work are requested to send the chart of any particular region, as soon as possible after its revision, to the address given below.

Harvard College EDWARD C. PICKERING.
Observatory, Cambridge, Mass. (U.S.) Dec. 17, 1884.

VERY LARGE METEOR.

Sir,—The outburst of a superb fireball at 5h. 35m. on 18th December last deserves notice, as being probably the largest I have ever witnessed. The illuminated surface was equal in apparent dimensions to the moon, but somewhat oval in form. Point of appearance about 1° N.W. of θ Bootis. Passed about 5° west of η Ursæ Majoris. Brilliant white colour.

Faithfully yours,
S. J. JOHNSON.

Melplash Vicarage,
Bridport, Jan. 3.

REFRACTOR v. *REFLECTOR.*

In reply to Mr. Newall's letter in the Register of last month, in which he recommends that the size of a telescope should be given in areas rather than in diameters, I would submit that the diameter is preferable; for in addition to its being in such constant use, it raises more readily in the mind the size of the instrument to which reference is made.

With regard to the want of proportion between the areas of four inches diameter to five, and five inches to six and a half, the wording of my letter makes it plain that this had been considered —the word "estimated" having reference to the state of the silver, which was a rubbed-up, seven years old film, as well as to the diameter of the stop.

When next the mirror is silvered I will try the experiment again, and, as suggested by Mr. Newall, with the same eye-piece.

NATHL. E. GREEN.

SATURN.

Sir,—In your January number, Mr. Hopkins remarks that occultations of small stars by Saturn should be looked for. At the last meeting of the Society I read a note of an observation made Nov. 23, concerning an illumination of the crape ring in the following elongation, due I believe to a star seen through it. This observation was literally thrust upon me, for I had been engaged on other work, and before closing my observatory turned the 10-inch on Saturn really for the purpose of examining the exact position of Mimas. Whilst so engaged my eye was continually annoyed by what, at first, I thought was a ghost in the

eye-piece. Finding it so perpetually on the particular part of Saturn above indicated, I changed it for my occulting eye-piece described in the monthly notices for Dec. Hiding the ball behind one of the shutters, I found this light was not imaginary, for I could now plainly see it was unusually illuminated. Watching it for about 10 minutes it vanished and the ring assumed its normal aspect. Time then 11.35 within a minute or minute and a half, for I gazed some little time after the vanishing moment to see whether the illumination returned again. Of course the observation was very unexpected, so I cannot be sure it lasted 10 minutes, but I noted it in my observatory book to be about that duration.

At the Society I stated the excess of light might be possibly due to a satellite whose orbit was *within* the rings, between them and the ball. Mr. Ranyard thought I meant one of those *outside* the rings, and stated he considered it impossible for one to appear in that situation, which is undoubtedly true, owing to the present position of Saturn. It is highly improbable that a satellite should be situated between the ball and rings, and yet never have been noticed ere this, unless, as was suggested to me after the meeting, possibly it can only be seen on rare occasions, and then only at elongation, like Mimas. The finding of four stars in or about the path of Saturn, seems to point to the star theory, but the weather has been so bad that I have not been able to search accurately for them. Lastly, Mr. Bryant informs me it would take 31¼ sidereal minutes for a star to apparently travel from the ball to the outer limits of the crape ring where it joins ring B, provided it followed the line of the major axis, so it is quite possible I did see it for 10 minutes.

Before concluding, I should like to call the attention of observers to the comfort experienced by the use of the occulting eye-piece to which I refer above. When the field around a bright object has to be inspected, it is almost impossible so to do without some such an arrangement, and as the present one requires no clock it is of especial use to some amateurs. I sometimes use three of the shutters, shutting out all extraneous light; but of course, like all new instruments, it requires learning, and after six months use I can confidently recommend it. I hope to derive much benefit from it in the coming occultation of Aldeboran.

EDMUND J. SPITTA.

Ivy House, Clapham Common.

REVIEWS.

Proof of the Rotation of the Earth by the descent of bodies from great heights

This subject is often referred to in astronomical treatises. Price, in the third volume of his *Infinitesimal Calculus*, writes "of a lofty and vertical tower, the top is of course further from the centre of the earth than the bottom, and therefore as the earth rotates (from west to east) the horizontal velocity of the top is greater than that of the bottom. Let a heavy ball fall from the top; it will have the horizontal velocity of the top, and this is greater than that of the bottom: if, therefore, the ball falls on the west side of the tower, it will strike the tower before it reaches the earth; but if it falls on the east side of the tower, it will strike the earth at a small distance from the tower towards the east. These results have been actually observed, and therefore we infer the law of which they are the effects. And he returns to the subject in the fourth volume, giving an approximate formula for the deviation, which is found to vary as the cube of the time during which the body has been falling, and as the square root of the cube of the height from which the body has fallen. But the subject is investigated at great length in *The earth and its mechanism, being an account of the various proofs of the rotation of the earth, &c.* By Henry Worms, F.R.A.S., F.G.S. London: 1862. In this able work the earth's rotation is proved by the transmission of light, by the varying force of gravity in different latitudes, by the descent of bodies from great heights, by the apparent change of the plane of oscillation of the free pendulum, by the gyroscope, and by the precession of the equinoxes. In the fifth chapter the writer observes that "a direct and most convincing demonstration of the earth's diurnal motion is furnished by those experiments which establish the fact that balls truly turned and carefully dropped from a great height do not fall exactly in the perpendicular, but deviate towards the east."

"Newton, in 1679, was the first who proposed to prove the rotation in this practical manner. . . . For if the earth rotates, the velocity of rotation must be greater at the summit of a tower than at its base, and a body dropped from the former will move faster in an easterly direction throughout the whole of its descent than the base does, and therefore reach the ground a little in advance of the foot of the perpendicular."

On the 14th January, 1680, Dr. Hooke, secretary to the Royal Society, reported to the society the results of his experiments, that the balls fell to the south-east; but the smallness of the height—27 feet—rendered them inconclusive. The deviation, in fact, could scarcely have exceeded the two-hundredth of an inch.* After this no more experiments of this kind were performed for a period of 112 years, when "Guglielmini, a geometer of Bologna, commenced making experiments in the tower degli Asinelli, which is 320 feet high. After many unsuccessful trials and various alterations in his apparatus, he gave up working in the day because he discovered that the traffic caused the tower to vibrate, and selected nights when the air was calm. The liberation of the balls was effected by burning the threads by which they were suspended, and this was never done until they had entirely ceased to oscillate, as observed by means of a microscope. The balls fell upon a cake of wax spread upon the ground, and the holes made by them nearly coincided; the greatest

* We have throughout converted the line and the French foot used in this work into English measures. A line being the 0·0888 of an inch, and the French foot = 12·789 inches.

distance between the two centres scarcely amounting to ·04 inches. The persevering philosopher finally dropped a plumb-line in August, 1791, but was obliged to wait till February, 1792, before it came to a state of perfect rest. He then found that the mean deviation of the sixteen balls he had dropped was 0·66 inch east, and 0·47 inch south. These results were received by the scientific world with the greatest interest, when Lalande announced that Laplace's theory gave only a deviation of 0·44 inch to the east and none to the south. This striking difference of theory and observation was at once explained by a trifling circumstance, overlooked in the otherwise most accurate experiments of Guglielmini, but the explanation overthrew at the same time all their force of demonstration. The difference was occasioned, first, by a local disturbance of the air, owing to the numerous perforations in the walls of the tower: and secondly, by a slight curvature of its structure. The balls were dropped in the summer, but the vertical was only ascertained six months afterwards, in the winter; and there is no doubt that the difference of temperature was sufficient to shift the point of suspension."

"Although these experiments did not achieve the result for which they were undertaken, yet they were the precursors of others, which furnished most satisfactorily the required proofs. Dr. Benzenberg commenced in 1802 a series of experiments in St. Michael's Tower in Hamburg, which is 428 feet in height, and most suitable for the purpose; for an uninterrupted fall of 362 feet may obtained, exceeding that in the Asinelli and in St. Paul's. But as only 255 feet of the tower, from the base upwards, were completely closed, Benzenberg chose a height of 250 feet for his investigations, in order that even strong winds might not influence the result. The balls used were 1·6 inch in diameter, and made of equal parts of lead and tin, with the addition of a little zinc, an alloy which is easily turned and polished. They were tested by being placed upon quicksilver, and those which betrayed eccentricity in floating were rejected.* . . . The suspending thread was cut, for burning might have caused a slight draught and an oscillation of the ball before being detached; and though he could not obtain permission to work at night, yet the vibrations of the tower were not perceptible, and the weather proving very favourable, his experiments may be looked upon as accurate. The last 31 balls he dropped fell as follows: 21 towards the east, 8 towards the west, 2 neutral, 16 towards the south, 11 towards the north, 4 neutral. [A table follows with the special results of the 31 experiments.] The sum of the deviations to the south was 8·22 inches; that of the deviations to the north 4·12 inches; difference = 4·10 inches. The sum of the deviations to the east was 15·50 inches, that of the deviations to the west 4·48 inches; difference = 11·02 inches. These numbers divided by 31 give respectively a mean deviation of 0·35 inches to the east, and 0·13 inches to the south. But the theory of Laplace does not give any southerly deviation, nor do the fundamental formulæ of Gauss allow any." Investigating the question on physico-mathematical principles, and taking account of the resistance of the air, Mr. Worms finds that no southerly deviation can exist, even if the experiment be made in air; and considering also the angles of inclination of the directions of gravity towards the falling body, which are, however, small, he arrives at the expression for the eastern deviation of the fallen ball, $\frac{4\pi t(h - \frac{1}{2}\Delta)\cos. L}{3r}$, where t is the time of descent of the ball, h the space of descent, Δ the excess of descent *in vacuo* above that in air, L the latitude of the place, r the

* Mr. Worms gives a figure representing the mode of suspension.

sidereal time of the earth's rotation. In the case of St. Michael's Tower, these quantities were :—$t = 4^s 3$, $h = 250$ feet, $\Delta = 9{\cdot}3647$, $L = 53° 33'$, $\tau = 86164^s{\cdot}1$, and the result of theory is 0·342 inches. Observation gave 0·355 inch, the difference being 0·013 inch. This result shows that as regards the easterly deviation, theory and observation agree, but there still remains a southerly deviation of 0·13 inch, which is not accounted for by theory."

Benzenberg repeated his experiments in an abandoned coal-pit, at Schlebusch, in the county of Mark, Westphalia, lat. 51° 25', and about 281 feet deep, of which about 279 feet were available for the free descent of the balls. Here $t = 4^s 164$, and $\Delta = 11{\cdot}4248$ feet, and the above formula gives the deviation to the east 0·412 inches. The new experiments were 29 in number. They were made in October, 1804, all precautionary arrangements having been made, such as covering the entrance of the pit and blocking up its lower outlets. The sum of the deviations in the accompanying table is as follows :—North 11·011 inches, south 9·146; difference 1·865. This divided by 29 gives as a mean result for a single ball a northerly deviation of 0·064 inch. East 16·8286, west 3·7296; difference 13·0990, which, divided by 29, gives mean easterly deviation 0·452. The small amount of northerly deviation is obviously due to the errors which can never be avoided in practice. On the other hand, the deviation towards the east, given by observation, approaches so closely to the theoretical result, 0·412, calculated upon the hypothesis of the earth's rotation, that the latter is confirmed, and the more so as no other reason can be assigned for the aggregate deviation of all the balls towards the east exceeding half the sum of the eastern and western deviations."

"An additional and very strong confirmation has since been furnished by the favourable result of a long series of similar experiments made by Professor Reich in the year 1831, in the mines of Freiberg. The depth he could command for the free descent of the balls was 520 feet, nearly double that of Schlebusch, the number of trials he made amounted to 106, and the mean result gave only a slight southerly deviation, viz., 0·041 inch., and an easterly deviation of 0·916 inch which differs in defect by 0·165 inch from the results of theory. Here $t = 6^s{\cdot}0098$, $\Delta = 61{\cdot}021$ feet, $L = 50° 53' 23''$, and the formula gives 1·081 inch for the result, by theory, of the eastern deviation."

Mr. Worms says in conclusion—"We cannot conclude this chapter without expressing a wish that some scientific man, animated by the truly practical spirit so often met with in Englishmen, may repeat these delicate and arduous experiments. This country offers singular local advantages for the prosecution of such researches. We have deep mines and high buildings, and we only require an enterprising and persevering philosopher to corroborate by his testimony the results of those inquiries which, though emanating originally here, have as yet only found successful votaries abroad."

In the second part of his book Mr. Worms presents the theory of the motion of falling bodies by Laplace and Gauss, with regard to the rotation of the earth.

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
Feb. 1	+65 1 E.	Feb. 11	−56 37 E.	Feb. 21	+ 1 40 M.
2	52 51	12	68 48	22	−10 29
3	40 41	13	80 57	23	22 40
4	28 32	14	+86 52 M.	24	34 50
5	16 21	15	74 42	25	47 0
6	4 12	16	62 32	26	59 11
7	− 7 58	17	50 23	27	71 22
8	20 7	18	38 12	28	83 32
9	32 18	19	26 2		
10	44 28	20	13 51		

Moon farthest from the Earth, Feb. 9, 12h.
(in new Greenwich time, „ 10, 0h.)
,, nearest the Earth „ 25, 12h.
(in new Greenwich time, ,, 26, 0h.)

THE PLANETS FOR FEBRUARY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	19 25 54	S. 22 1	6″·0	22 34·8
	9th	20 12 12	S. 21 4	5″·6	22 49·5
	17th	21 2 7	S. 18 45½	5″·2	23 7·8
	25th	21 54 3	S. 15 1½	5″·0	23 28·2
Venus ...	1st	19 31 15	S. 21 54	10″·9	22 40·1
	9th	20 13 33	S. 20 22	10″·7	22 50·8
	17th	20 54 50	S. 18 12	10″·4	23 0·6
	25th	21 34 56	S. 15 28	10″·4	23 9·1
Jupiter ...	1st	10 21 51	N. 11 31	41″·5	13 32·2
	9th	10 18 10	N. 11 53½	41″·8	12 57·1
	17th	10 14 13	N. 12 11	41″·9	12 21·7
	25th	10 10 13	N. 12 39½	41″·8	11 46·3
Saturn ...	1st	5 6 11	N. 21 32	17″·7	8 17·4
	9th	5 5 28	N. 21 33	17″·5	7 45·2
	17th	5 5 16	N. 21 34½	17″·2	7 13·6
	25th	5 5 34	N. 21 37	17″·0	6 42·4
Uranus ...	6th	12 10 23	S. 0 17	4″·0	15 0·8
	18th	12 9 2	S. 0 8	4″·0	14 12·3
Neptune ...	2nd	3 14 18	N. 16 12½	...	4 32·9
	18th	3 14 43	N. 16 15	...	3 31·3

Mercury rises an hour and ten minutes before the sun at the beginning of the month, the interval decreasing.

Venus rises about an hour before the sun on the 1st, the interval decreasing.

Jupiter rises about an hour and three quarters after sunset at the beginning of the month, the interval decreasing.

Saturn sets about four hours and a half after midnight on the 1st, the interval decreasing.

ASTRONOMICAL OCCURRENCES FOR FEBRUARY, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Saturn —
Sun	1	8 1 8 57 15 29 16 41 17 26 18 28 18 29 19 25	Occultation of *d* Leonis (5) Reappearance of ditto Occultation of B.A.C. 3836 (6) Reappearance of ditto Occultation of 75 Leonis (5½) Reappearance of ditto Occultation of 76 Leonis (6) Reappearance of ditto	1st Ec. D. 2nd Sh. I. 1st Oc. R. 2nd Tr. I.	14 33 32 17 15 17 15 18 6	8 17·4
Mon	2		Sidereal Time at Mean Noon 20h. 51m. 22·16s.	4th Ec. D. 1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E. 4th Oc. R.	10 8 28 11 48 12 12 14 8 14 31 18 25	8 13·4
Tues	3		Sun's Meridian Passage 14m. 6·42s. after Mean Noon	1st Ec. D. 1st Oc. R. 2nd Ec. D. 2nd Oc. R.	9 1 54 11 41 12 9 27 15 49	8 9·3
Wed	4	15 13 16 11	Occultation of B.A.C. 4591 (6) Reappearance of ditto	1st Sh. E. 1st Tr. E.	8 37 8 57	8 5·3
Thur	5			2nd Tr. I. 2nd Sh. E. 2nd Tr. E. 3rd Sh. I. 3rd Tr. I. 3rd Sh. E. 3rd Tr. E.	7 13 9 28 10 8 13 18 14 37 16 57 18 13	8 1·3
Fri	6	10 37	☾ Moon's Last Quarter			7 57·2
Sat	7			1st Sh. I.	19 14	7 53·2
Sun	8	14	Neptune at quadrature with the Sun	1st Ec. D.	16 27 6	7 49·2
Mon	9			3rd Oc. R. 1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	7 53 13 42 13 56 16 2 16 16	7 45·2
Tues	10			1st Ec. D. 1st Oc. R. 2nd Ec. D. 4th Sh. I. 2nd Oc. R.	10 55 31 13 25 14 45 57 17 34 18 5	7 41·3
Wed	11	0 12	Conjunction of Mars and Sun Conjunction of Venus and Mercury 0° 44′ S.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	8 11 8 22 10 31 10 41	7 37·3
Thur	12	22 23	Conjunction of Moon and Venus 5° 9′ S. Conjunction of Moon and Mercury 4° 30′ S.	1st Oc. R. 2nd Sh. I. 2nd Tr. I. 2nd Sh. E. 2nd Tr. E. 3rd Sh. I. 3rd Tr. I.	7 51 9 6 9 26 12 2 12 21 17 16 17 54	7 33·3
Fri	13		Sidereal Time at Mean Noon 21h. 34m. 44·25s.			7 29·3
Sat	14	14 21	● New Moon Illuminated portion of disc of Venus = 0·944	2nd Oc. R.	7 12	7 25·4

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Saturn.
Sat	14		[*14th day continued.*] Illuminated portion of disc of Mars = 1·000			
Sun	15			1st Ec. D.	18 20 49	7 21.5
Mon	16	16	Conjunction of Moon and Mars 4° 30′ S.	2nd Ec. D. 3rd Oc. R. 1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	7 21 48 11 9 15 37 15 40 17 56 18 0	7 17·5
Tues	17		Saturn's Ring: Major axis = 43″·25 Minor axis = 19″·56	1st Ec. D. 1st Oc. R. 2nd Ec. D.	12 49 16 15 8 17 22 29	7 13·6
Wed	18	20	Opposition of Jupiter to the Sun	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	10 5 10 6 12 25 12 25	7 9·7
Thur	19		Sun's Meridian Passage 14m. 1·18s. after Mean Noon	4th Oc. D. 1st Oc. D. 4th Ec. R. 1st Oc. R. 2nd Tr. I. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	4 3 7 15 8 41 52 9 34 11 38 11 40 14 33 14 36	7 5·8
Fri	20	7 41 8 1	Occultation of 38 Arietis (5) Reappearance of ditto	2nd Tr. E. 1st Sh. E.	6 51 6 54	7 1·8
Sat	21	22 31	☽ Moon's First Quarter	2nd Oc. D. 2nd Ec. R.	6 32 9 33 41	6 57·9
Sun	22	5 17 5 50 20	Occultation of α Tauri (1) Reappearance of ditto Conjunction of Moon and Saturn 3° 44′ N.			6 54·1
Mon	23	11 3 12 2	Occultation of 130 Tauri (6) Reappearance of ditto	3rd Oc. D. 3rd Ec. R. 1st Tr. I. 1st Sh. I.	10 49 14 49 46 17 24 17 31	6 50·2
Tues	24			1st Oc. D. 1st Ec. R.	14 32 16 57 55	6 46·3
Wed	25			1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	11 50 12 0 14 9 14 19	6 42·4
Thur	26	3 59 4 24 16 1 16 53	Occultation of B.A.C. 2872 (6) Reappearance of ditto Occultation of α Cancri (4) Reappearance of ditto	1st Oc. D. 1st. Ec. D. 2nd Tr. E. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	8 58 11 26 24 13 51 14 14 16 47 17 10	6 38·6
Fri	27	17 24 17 37 18 5 18 45	Occultation of B.A.C. 3407 (6) Reappearance of ditto Occultation of π Leonis (5) Reappearance of ditto	2nd Sh. I. 1st Tr. E. 1st Sh. E. 4th Tr. I. 4th Sh. I. 4th Tr. E. 4th Sh. E.	6 28 8 35 8 48 9 38 11 34 14 11 16 17	6 34·7
Sat	28	16 11 59 13 4 0	○ Full Moon Occultation of 35 Sextantis (6) Reappearance of ditto Conjunction of Moon and Jupiter 4° 27′ N.	2nd Oc. D. 2nd Ec. R.	8 48 12 10 5	6 30·9

Books received.—Harvard College. Report of the Committee of Standards of Stellar Magnitudes.—Astronomische Nachrichten.—Sidereal Messenger.—Boletin de la Instucion Libre de Ensenanza, Madrid.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To June, 1884.
Tupman, Col.

To Dec., 1884.
Compton, A. J. S.
Erck, Dr. W.
Green, N. E.

To June, 1885.
Banks, Mrs.
Gooch, Miss
Lancaster, J. L.
Lancaster, W. L.

To Dec., 1885.
Arcimis, A. de
Barber, J. T.
Clegram, W. H.
Collingwood, E. J.
Davies, Rev. R. P.
Garnett, W.
Fielding, G. E.
Joynson, J.
Jefferies, J.
Knolt, G.
Numsen, W.
Redpath, H. G.
Riddiford, G. F.
Terby, Dr.

To Jan., 1886.
Hutchings, Rev. R. S.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to John C. Jackson, Chief Office, London.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 267. MARCH. 1885.

ROYAL ASTRONOMICAL SOCIETY.

Session 1884—85.

The Annual meeting was held at the Society's Rooms, Burlington House, on Friday, Feb. 13th, at three in the afternoon.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut-Col. Tupman.

The following gentlemen were balloted for and duly elected Fellows of the Society:

Rev. Edwd. Allen, M.A., Castlechurch Vicarage, near Stafford.

Rev. John Burgess, 90, Cheriton Road, Folkestone.

P. Edward Dove, 9, Argyll Street, Regent Street, W.

Mr. Knobel read the minutes of the last annual meeting.

Mr. Lecky read the auditor's report, which showed that the society has an income of £402 9s. 2d., derived from investments, and an income of about £900 derived from annual contributions and admission fees. The balance at the bankers on the 1st January, 1885, was £338 9s. 2d.

Mr. Knobel read parts of the Report of the Council, which showed that twenty-five new Fellows had been elected during the year, and that the Society now consists of 609 Fellows and 49 associates. Among the lives of deceased Fellows given in the "Annual Report" is a life of Mr. Todhunter, which has been written for the Society by Mr. Routh. It stated that Mr. Todhunter was born in 1820; he was the second son of a Congregationalist minister at Rye. After leaving school he became an

assistant master in a school at Peckham, and at the same time attended the evening classes at University College, and amongst others the lectures of Prof. De Morgan. He seems to have come under the fascination which so many of the pupils of that great teacher experienced: through the rest of his life his admiration for De Morgan was unbounded. He obtained great distinction at the University of London, carrying off the honours at the degree examinations for B.A. and M.A. He afterwards entered at St. John's College, Cambridge, and became senior wrangler and first Smith's prizeman in 1848. Soon after his degree he established himself at St. John's College as a mathematical tutor and lecturer, but he afterwards gave up all share in college tuition, and devoted himself to the work of writing text-books. Mr. Routh says, His books conduct the student from the beginning through a long course of mathematical learning, and they have materially affected the education of this generation. His reputation in the future will undoubtedly rest on his histories of mathematics, for the fashion of elementary text-books will pass away, and a new generation will like a new arrangement of old things. The most important of these histories are the history of the calculus of variations, the history of the theory of probability, from the time of Pascal to that of Laplace, and the history of the mathematical theories of attraction and the figure of the earth.

In the Council's report upon the progress of astronomy during the past year, the Cordoba Zone Catalogue is referred to. Dr. Gould originally planned a private expedition to South America, with the object of completing a general revision of the whole heavens by zone observations similar to those which had been carried out in the Northern hemisphere by Bessel and Argelander. Dr. Gould's private expedition was adopted by the Argentine nation, and became the germ of its national observatory. In the catalogue which has just been printed there are 105,240 separate observations, giving the positions of 73,160 different stars. Prof. Safford's investigations of the corrections of Greenwich planetary observations from 1762 to 1830 is also referred to, as well as Baron Dembowski's double-star measures, of which the second volume has been published during the year.

Five comets have been under observation during the year 1884. The first of these was discovered on Jan. 7, by Mr. David Ross, of Elsternwick, near Melbourne. The second was the Pons-Brooks comet, which was last seen on June 2 in New Zealand, 128 days after its perihelion passage. The third comet was first seen by Mr. E. Barnard, of Nashville, on the night of July 16th, while sweeping in the south-west; it remained during the whole

of its visibility an inconspicuous object, but there were alternations in brightness from day to day, which were not uniform, and did not correspond with the law of reflected light. This comet evidently has a short elliptic orbit, with a period of about five and a half years. Comet IV. was discovered by Max Wolf, of Heidelberg, on September 17, in the constellation of Cygnus. Like the preceding comet, this also has an elliptic orbit of short period, probably about 2,460 days. On the 13th December, Encke's comet was reobserved by Dr. Temple at Arcetri. In addition to these comets, Brorsen's comet passed its perihelion in the middle of September, but it seems to have escaped observation.

Nine minor planets were discovered during the year 1884, which brings their number up to 244. Of these nine, six were discovered by M. Palisa, of Vienna.

The President, in presenting the gold medal of the Society to Dr. Huggins, said: Gentlemen,—You have already been informed in the annual report that the Council have awarded the gold medal of the Society to Dr. William Huggins for his researches on the motions of stars in the line of sight, and on the photographic spectra of stars and comets. It now becomes my pleasing duty to explain to you briefly, in a general manner, the principal grounds by which the Council have been influenced in coming to their decision. Before doing so, however, perhaps I may be permitted to remark that no one can possibly refer to the printed list of our medallists without being impressed with the wideness of scope available to the astronomical inquirer, as illustrated by the great diversity of subjects for which the medal has been awarded by the respective Councils in past years. A glance at the names contained in the printed list shows clearly that the medals of the Society have been presented to astronomers without regard to nationality or to subject, for their original researches either in the mathematics, the ordinary observing labours, or in the literature of astronomy. It must be borne in mind that this, the highest scientific honour that the Society can bestow, has never been presented to any one before his claims have been well weighed by those who, for the time being, have been the Society's representatives. The chief object of the Council, so far as my experience goes, has always been the endeavour to recognise the original works of astronomers generally whenever any specially high merit is exhibited, or a definite advance on some point in astronomy is made; and I believe in a remark made by Sir George Airy that "Scarcely ever has it occurred that in looking back to the award of our medals we could say that any important subject had been omitted

which ought to have been taken into consideration, or that a better choice could have been made than that which we have made." As you are all doubtless aware, the Society's gold medal was awarded in 1867 to Dr. Huggins, jointly with Dr. W. Allen Miller, for their researches in astronomical physics. The grounds for this award are most ably and eloquently given in the address of the President.

Dr. Huggins's first published memoir on his method of determining the relative radial motion of the earth and stars appeared in 1868, though the subject had previously occupied his attention, as well as that of Dr. Miller, so early as 1862, while they were engaged on the comparison of the bright lines of terrestrial substances with the dark lines in the spectra of a few of the principal stars. Owing, however, to the insufficiency of the apparatus in these early and delicate experiments, the first results were on the whole of a negative character, though enough evidence was obtained to show that better instrumental means only were required to test the method and bring about some definite conclusion. Sufficient, however, was done in the early part of 1868 to enable Dr. Huggins to present with some confidence to the Royal Society some observations on a small change of refrangibility which he had observed in a line in the spectrum of Sirius, when compared with a line of hydrogen. This small displacement indicated that the star was receding from the earth in the direction of the line of sight with a velocity of about twenty-nine miles per second. Dr. Huggins does not claim in any way to be the first to suggest the theoretical idea of using the spectroscope for determining the radial motions of stars to or from the earth. In his first memoir he refers to the researches of Doppler, Fizeau, Clerk-Maxwell and others, who had considered the theoretical question; but it was reserved for Dr. Huggins to make such measures practicable, and to obtain results which have been amply confirmed by observations made at the Royal Observatory, Greenwich, and the Observatory at Potsdam.

The mapping of the photographic spectra of stars, as carried out by Dr. Huggins, is a research of great delicacy, as a perfect definition of the bands is absolutely necessary to allow of the accurate measurement of their respective wave-lengths. It may be supposed that to obtain this satisfactorily the most careful manipulation is requisite, when it is considered that the photographic plates are only 1½ in. long by ½ in. wide, and the length of the photographic spectrum between the lines G and P in the ultra-violet is only about ½ in. The definition is usually very good, and the photographs can very easily be examined, and the position of the lines measured under a low-power microscope; and, though

the solar spectrum on the plate is only ½ in. in length, about fourteen lines may be counted between the lines H and K. The difficulty of the observation arises partly on account of the small quantity of light at the disposal of the observer, and partly on account of the extreme care required to obtain accurate comparisons of the star spectrum with the spectra of known substances, in order to ascertain whether the presence or absence of such and such substances may be detected in the stars. The general conclusion, from the discussion of the different spectra, appears to be that the results may conveniently be divided into two groups, or types, with slight variations in individual cases—namely, into white stars showing a typical group of twelve dark lines belonging to the same substance, probably hydrogen, and another group exhibiting a spectrum of which our own sun may be taken as a type. In addition to these stellar photographs, Dr. Huggins has also taken successful photographs of the spectra of Venus, Mars, and Jupiter, together with a broad daylight spectrum for comparison. They, however, fail to show any additional absorption lines, or any modifications of the solar light. Photographs of the light of the moon from limited areas of the lunar surface have been taken under different conditions of illumination, and also during partial eclipses of the moon. The photographs have been examined and the lines measured by a micrometer attached to a low-power microscope. The measures were then reduced to wave-lengths by the help of solar and terrestrial spectra, with the assistance of Cornu's map of the ultra-violet part of the spectrum, and of Mascart's determinations of the wave-lengths of cadmium. The spectra of Sirius, η Ursæ Majoris, Spica, α Aquilæ, α Lyræ, α Cygni, and Arcturus admit of easy comparison by reference to the map which is laid down on the scale of that of Cornu.

I have already briefly referred to Dr. Huggins's direct eye-observations of the spectra of several comets, and to the important deductions he has obtained from them relating to their physical constitution, but I am now desirous to draw your particular attention to a successful photograph of the spectrum of Comet I., 1882 (Wells), in which the cometary bands differ considerably from those observed in the spectra of previous comets. These bands are exhibited with great clearness in the photograph, an enlarged copy of which may be found in the *Proceedings*, R. S., vol. xxxiv. p. 149. This photograph was taken through one half of the slit on the evening of May 31, 1882, with an exposure of an hour and a half. For the convenience of accurate comparison a spectrum of α *Ursæ Majoris* was taken on the same plate through the other half of the slit. It shows a strong continuous spectrum without the appearance of any of

the Fraunhofer lines, extending from about F to a little beyond H. The spectrum differs greatly from that of Comet I. 1881, as also from the spectra of the comets (about twenty) which had been previously examined spectroscopically, and contains bright lines indicating the presence of vapour of sodium, and also some other bright lines and groups of lines. "The continuous spectrum which extends from below F to a little distance beyond H contains at least five brighter spaces, which are doubtless groups of bright lines, though it is not possible in the photograph to resolve them into lines. These places of greater brightness can be traced beyond the border of the continuous spectrum on the side which corresponds to the coma of the comet on the side next the Sun. The light from this part of the comet gave a very much fainter continuous spectrum, for on the photographic plate it appears to be almost wholly resolved by the prisms into these bright groups. One or two fainter groups are suspected to be present, but they are too indistinct to admit of measurement." The beginning and ending of the bright groups are very faint, and the estimated brightest parts only are capable of being measured. In the photographic spectrum of the great nebula of *Orion*, obtained on March 7, 1882, after an exposure of forty-five minutes, may be noticed five bright lines, as well as a narrower continuous spectrum which Dr. Huggins considers may be due to stellar light. In his previous eye-observation of the spectrum of this nebula he had found four bright lines, the brightest being coincident with the less refrangible component of a strong double line in the spectrum of nitrogen. The second line has a wave-length of 4957 of Angström's scale, the third and fourth lines being coincident with, as I have already stated, two lines of hydrogen. In the photograph these lines are faint, but they can be satisfactorily recognised and measured. In addition to these the photographic spectrum shows a comparatively strong line in the ultra-violet, corresponding nearly to ζ of the typical spectrum of white stars. Dr. Huggins's successful application of photography to the subject of these inquiries is now so far acknowledged to be an important astronomical achievement that it is hoped that others will follow his example. But any attempts to follow in his footsteps will certainly end in failure unless the observer is in possession of instruments of the highest class as well as the necessary zeal for the work. This inquiry into the probable physical constitution of individual stars ought not to be considered completed by Dr. Huggins's experiments, but rather it should be looked upon as the commencement of a research to include the examination and classification of the spectra of all the large stars in both hemispheres.

That this will be done sometime in the future I have not the least shadow of a doubt. In the meanwhile we are all able to appreciate the excellent work which our Foreign Secretary has initiated and accomplished.

The President, then delivering the medal to Dr. Huggins, addressed him in the following terms:—Dr. Huggins,—I have great pleasure in presenting you with this medal, which has been honourably awarded to you by the Council. I trust you will accept it as the highest acknowledgment of your valuable services to our science which it is in their power to bestow. This is not the first time that your devotion to astronomy has been recognised from this Chair, and I hope that it is not the last, for we still find you carrying on with success your important and delicate researches. May your health be long preserved, and may astronomy long continue to receive the benefit of your talents, which I have no doubt will be the means of still further adding to our present knowledge of the constitution of the universe.

The Earl of Crawford proposed that the Report of the Council be received and adopted, and that it be printed and circulated in the usual manner, together with the Report of the Auditors and the President's address.

Professor Forbes said: I have much pleasure in seconding the motion.

The motion was put and carried.

Mr. Waters was then called upon to move a motion of which he had given notice. He said: I rise, in accordance with notice I have given to the Council, to move a resolution asking the Council to take into consideration the alteration of Bye-Law 44. The suggestion I have to make is that we should alter the time of holding our ordinary meetings from 8 o'clock to 5 o'clock in the afternoon. I believe the present hour was fixed upon about 50 years ago; and I think it will be admitted that now, at any rate, we may consider whether a more convenient time may not be fixed. The evening meetings are very inconvenient to many members who live in the country, who are obliged to stay in London for the night, or be exposed to a late train journey home. The meetings are also not particularly convenient to those who live in London, and who are engaged, or ought to be engaged, elsewhere at that late hour of the evening. Moreover, I think the evening meetings prevent the attendance of some of our most eminent and older Fellows, who might be inclined to attend if the meetings were held in the afternoon. Among the minor reasons for a change I may mention that the meetings at the Royal Institution are held on Friday evenings at the same

time as ours, and a great many of our Fellows are members of the Royal Institution; and that is a point that might be considered. If this Society should decide on any change, we may perhaps be able to judge of what the effect will be, inasmuch as the Royal Society some years ago made a similar alteration, and the result has been a considerable increase in the attendance; and those Fellows of the Royal Society with whom I have had an opportunity of speaking have expressed their satisfaction at the change. I will not occupy the time of the meeting longer, and will only add that I have brought the matter forward not necessarily for settlement at the present meeting, but more with the idea of eliciting an expression of opinion and bringing it forward again for future consideration. With that object I will propose: "That Bye-Law 44 be altered by substituting 17 hours for 8 o'clock."

Mr. Freeman: I have very much pleasure in seconding that motion. When I resided at Cambridge I found it inconvenient to come to town for an evening meeting, because it was next to impossible to get back the same evening; and now that I live some 45 miles from London I find it still more inconvenient to come to the meetings as I should desire. You must take into account that those who live in the country not only have to pay their railway fare to London and back, but if they have to stop in town for the night they have a heavy hotel bill besides; and on both these accounts I was delighted to see that this proposal was to be made by Mr. Waters. I hope we shall not shrink from coming to a division, for I came expressly to record my vote, though if I cannot do so within four minutes I shall not have the opportunity of supporting the resolution.

Mr. Ranyard: I have noticed that at these afternoon meetings of the Society one does not see the same faces as at our evening meetings; and it seems to me rather unfair that the gentlemen who are able to come here in the afternoon should decide a question like this for the Fellows who come here in the evening. I should imagine that there are a great many Fellows who are occupied during the day and who would not be able to come in the afternoon. And I think when a rule has been carried out for half a century there should be some very decided majority of feeling shown by the Society before a change is made; and therefore unless there is such a decided majority shown at this meeting, and the resolution is confirmed at an evening meeting, I think it would be unwise to change our custom. I should also like to make a little criticism as to the words of the motion which Mr. Waters has made. He proposes that the words "17 hours" should be used in our bye-laws instead of

5 o'clock; that is to say, he proposes that the Royal Astronomical Society should throw their weight in favour of using the somewhat pedantic language of the new nomenclature of time—namely, to count the hours from 0 to 24 for the purposes of civil life. I think a very good case should be made out before such a course is taken by the Society. For my own part, I do not see the necessity of making any such change for the ordinary purposes of life; it is likely to confuse our friends, and we shall have to think some time before we make up our minds whether we can keep an appointment. There is also the difficulty of numbering clock-faces according to the new method. It will certainly be more difficult to read the time by them than at present. I, therefore, think we had better hesitate before we take the lead in such a great alteration as this.

Mr. Knobel: I should like to place a few facts before the Society with regard to the effect of Mr. Waters's proposal, so that the Fellows may understand what it involves. It is obvious that in conducting the affairs of the Society, the meetings of the Council and of Committees must take place on the day of the ordinary meeting of the Society, and therefore we must take it for granted that, if the change is made, the meetings of the Council and of Committees will take place before the proposed meeting at 5 o'clock. The Council meeting occupies from two to three hours, and therefore we must allow three hours. At our December meeting the Council have to consider the claims of nominees for the medal and the preparation of the Council list, and that always takes a great deal of time, and three hours must be allowed for that purpose. It would necessitate the Council meeting at 2 o'clock in order to get done by 5 o'clock. At several of our meetings during the year Library Committees have to be held, which take about three-quarters of an hour; and therefore the Library Committee would have to meet at a quarter past 1 o'clock, in order that the members may be present at the 2 o'clock meeting of the Council, and at the proposed 5 o'clock meeting the business would go on until 7 o'clock at the earliest; and there will be a quarter or half-an-hour's business afterwards with the Assistant Secretary, arising out of the general business, and therefore the business would not be over before half-past 7; that is to say, the President and officers of the Society might be uninterruptedly at work, without any time for refreshment, from a quarter past 1 until half past 7. The meeting should, I think, clearly understand the issues involved before voting on this question.

The Earl of Crawford: For certain reasons I should have been in favour of Mr. Waters's motion, but I think the balance of

feeling is against the suggestion. Five o'clock is certainly too early. If he had mentioned 19 hours—7 o'clock—I should have been with him, because at our present meetings at 8 o'clock many interesting matters are often left unread. But I think 5 o'clock would be too early, because few business men would be at liberty at that time if they are to have any refreshment after their work.

Mr. E. J. Stone: I should prefer 5 o'clock, as a resident in the country; but if you were to adopt 7 o'clock I do not think it would make much difference, and I do not think many of us would be able to go home afterwards. Though, individually, I should be strongly in favour of the change, I should not be in favour of a meeting like this taking the responsibility of making the change. If any idea of the kind is entertained, a circular should be sent round to all the members for an expression of opinion, and then you would be in a position to say whether there is a large majority in favour of the change. If the majority in favour is not large, I think no change should be made. With regard to the adoption of the term 17 or 19 hours, I must confess I look upon it as a little premature, and such a change in time should not be lightly made. If we are to adopt the new system —and I should be perfectly willing to carry it out thoroughly—I should like to have some time fixed at which we are all to begin. It appears that some gentlemen are in favour of it, and others are much opposed to it. It is very doubtful, indeed, as far as I can see, whether it really will be adopted by astronomers and fairly carried out, and if not, I hardly see why any change should be made. If we are to have one universal time, it should be adopted only after it has been deliberately accepted by a large majority of people; and when it is fairly carried out, our bye-laws would have to be accommodated to the change, but meantime we had better continue to speak of 5 or 8 o'clock until the change had been made.

The Astronomer Royal: I think it is a little unfortunate that Mr. Waters has mixed up two perfectly distinct questions. I think the less said about the "time" question at present the better, because I quite agree with Mr. Stone that discussion on that point is premature. We have not yet received any official notification of the proceedings at Washington, and therefore the question may be deferred. With regard to the other question, it is one that requires careful consideration before we make a change. Personally, I am disposed to think the change would be for the benefit of the Society, and that we should have the meetings better attended. As regards Mr. Ranyard's objection, it is true that this is an afternoon meeting, but I understand it

is not proposed to hold the meetings at 3 o'clock, but at 5 o'clock, and I think the majority of the Fellows who are engaged in the office hours of the day would be able to get to the meeting at 5 o'clock. (Cries of "No.") As a matter of fact, the meetings of the Royal Society, which are held at 4.30 p.m., are better attended now than when they were held at 8 o'clock in the evening. Of course it is possible that some members would not be able to attend, but we have to consider the balance of advantages and disadvantages. As regards Mr. Knobel's objection as to the shortness of time for the Council meetings which the change would necessitate, the Council meeting could be held earlier. I do not think the case is so bad as he has made out, for as a matter of fact the Council meetings are almost always over by 5.30 at present, and the proposal is to have the general meeting only half an hour before that time. It is not necessary to have the Library Committee, or any Committee, on a day when there is a very long Council meeting, such as the one in December, and I think it would be feasible to hold the Council meeting before the 5 o'clock meeting of the Society, and to have a short interval for tea before the meeting instead of after it. That is the practice of the Royal Society, and the meetings of their Council occupy only about half the time they did formerly, and I do not think that is an evil at all. Under these circumstances I would suggest that this question should be postponed, and that it be left for the Council to take up the matter, and endeavour to obtain the sense of the Society generally on the subject. (Hear, hear.)

The Rev. J. C. Jackson: I have great pleasure in seconding that motion, and at the same time I wish to show the Society a very curious old clock face. The Italian painters were satisfied with dividing the clock face into six hours.

Mr. Waters: I shall have great pleasure in agreeing to the suggestion of the Astronomer Royal. I brought the matter forward in order to elicit opinions on the subject.

Mr. Chambers: Before this question is actually disposed of to-day, I should like to suggest that though the matter be postponed for further consideration, yet that the opinion of the Fellows should be collected in a definite way. The suggestion I have to make is that reply post-cards should be sent to each Fellow, with some such question as this:—"Are you in favour of an alteration of the hour of meeting? and if so, would you prefer 5 to 8 o'clock?" In that way you will be able to gain a fair statement of the wishes of the 500 Fellows of this Society, which cannot be done at any meeting, owing to the wide area over which they are scattered. Personally I am strongly in favour of

an alteration to 5 o'clock, but unless a poll is taken in some such way as I suggest, I do not think it would be possible to learn the wishes of absentees.

Professor Adams: Might it not be possible to try the experiment of a change for two or three meetings, and then we should see whether the meetings would be better attended.

The Earl of Crawford: I do not think that would be possible, because the bye-laws say we shall hold the meeting at a definite time. I quite agree with Mr. Chambers that if the thing is to be done at all, it should be done thoroughly, and if we drop the subject now, and the Council will undertake to ventilate the matter thoroughly among the Fellows, that will be a proper course to pursue.

Professor Pritchard: I wish to second the motion of Mr. Chambers. It seems to me a very practical suggestion.

The President: I should like to say that the Royal Society before they made the alteration sent out circulars to each Fellow of the Society, and only one-half of them were returned, but a majority of that half was in favour of altering the time from 8·30 to 4·30 P.M.

Professor Adams: There would be one advantage attending the change. It would make the Council utilize their time better, I think. They would not be so likely to dawdle over their business. (Laughter.)

Some discussion took place as to whether Mr. Chambers' motion could be taken as an amendment to Mr. Waters' motion; and ultimately Mr. Waters formally withdrew his motion, and Mr. Chambers proposed his in the following terms:—"That the Council be requested, within the next eight weeks, to send a reply post-card to each Fellow, asking for a reply to the question whether the Fellow receiving the same is in favour or against an alteration of the hour of meeting from 8 to 5 o'clock."

The President put the resolution, and declared it carried.

Mr. Ranyard: If that question is disposed of, there is another matter I should like to mention. Mr. W. S. Franks has sent out a notice to some of his friends who have signed it, it runs thus: "We, the undersigned Fellows of the Society, respectfully request the Council to consider the advisability of altering the bye-laws so as to enable Fellows who cannot attend the annual general meetings of the Society to vote at such meetings by proxy."

Mr. Knobel: The requisition which Mr. Ranyard has read has been received by the Council to-day, and they will take it into consideration at their next meeting, and I think it is out of order to bring it up now.

Mr. Ranyard: I think I am not out of order, any more than Mr. Chambers was. I am not going to propose any alteration of the bye-laws, but I think it advisable that the opinion of this meeting should be taken on this important question. Mr. Franks has taken the opinion of several of his friends, and I understand that out of some ninety letters which he has received, eighty-three—or more than nine-tenths—of his correspondents agree that it would be desirable that the Council should take into their consideration the question whether voting by proxy should be introduced.

Mr. Knobel called attention to Bye-law 52, which provides that requisitions shall be considered by the Council, and that if the requisitionists are not satisfied with the decision the subject may then be discussed at a special general meeting.

Mr. Ranyard; Of course we have the power of recommending an alteration to the Council, for if five Fellows have the power of making such a recommendation to the Council, fifty of us have the power, and any meeting has power to discuss anything which the majority may consider a sufficiently important question. I therefore think I am in order in mentioning the matter.

Mr. Common: If the question that Mr. Ranyard is bringing forward involves an alteration of the bye-laws, it is distinctly out of order. When a ballot should take place is pointed out by the bye-laws, and to discuss any alteration without notice must be out of order.

Professor Adams: I think it is clear that the case under discussion is really a case clearly provided for under the bye-laws. A particular way of proceeding to an alteration of the bye-laws is pointed out there, and it seems to me that Mr. Ranyard is proposing a method of proceeding exactly the reverse.

Mr. Ranyard: As you have allowed speeches to be made, Mr. President, by those who conceive that I am not in order, you will, no doubt, allow me to reply before you give your ruling. My reply is, that I am not about to propose any alteration of the bye-laws. I am proposing to take the sense of the meeting with regard to a recommendation to the Council. There is nothing in the bye-laws which prevents this.

The President: Mr. Ranyard, you are out of order.

Mr. Ranyard: Will you, Mr. President, take the sense of the meeting as to whether this question is to be burked? (No, no.)

Lord Crawford: Will you allow me to say a word on this subject? As one of the signatories to the requisition, I am cordially in favour of voting by proxy, but at the same time I feel that Mr. Ranyard is not in order. I am on his side, but after what Mr. Knobel has read from the bye-laws, I think Mr. Ran-

yard has not a leg to stand upon, because it would be prejudicing a decision which is to be arrived at by the Council. If the Council refuse to grant our request, our course is very simple. We shall then have to call upon the Council to convene a special general meeting, when the subject will have to be discussed.

The matter then dropped.

Mr. G. F. Chambers, Dr. Mann, and Mr. Waters were appointed scrutineers of the ballot, and the following gentlemen were declared to have been elected.

President.

Edwin Dunkin, Esq., F.R.S.

Vice-Presidents.

J. C. Adams, Esq., M.A., LL.D., F.R.S., Lowndean, Professor of Astronomy, Cambridge.
Arthur Cayley, Esq., M.A., LL.D., D.C.L., F.R.S., Sadlerian Professor of Pure Mathematics, Cambridge.
Warren de la Rue, Esq., M.A., Ph.D., D.C.L., F.R.S.
E. J. Stone, Esq., M.A., F.R.S., Radcliffe Observer.

Treasurer.

A. A. Common, Esq.

Secretaries.

E. B. Knobel, Esq.
Lieut-Col. G. L. Tupman, R.M.A.

Council.

Capt. W. de W. Abney, R.E., F.R.S.
Sir G. B. Airy, K.C.B., M.A., LL.D., D.C.L., F.R.S.
J. Rand Capron, Esq.
W. H. M. Christie, Esq., M.A., F.R.S., Astronomer-Royal.
A. M. W. Downing, Esq., M.A.
J. W. L. Glaisher, Esq., M.A., F.R.S.
J. R. Hind, Esq., LL.D., F.R.S., Superintendent of the "Nautical Almanac."
George Knott, Esq., LL.B.
E. W. Maunder, Esq.
Rev. Chas. Pritchard, D.D., F.R.S., Savilian Professor of Astronomy, Oxford.
A. Cowper Ranyard, Esq., M.A.
Lieut.-Gen. J. F. Tennant, R.E., F.R.S.

THE LIVERPOOL ASTRONOMICAL SOCIETY.

At the monthly meeting, held on the 9th February, the President occupying the Chair, Mr. W. H. Sharp detailed some interesting experiments he had made with a view of re-determining the density of the earth. The method he had employed was a modification of the plan adopted by Cavendish; but instead of a pendulum hung horizontally from the centre he had used a pendulum hung after the fashion of a gate. Friction had been reduced to a minimum by the substitution of needle points for hinges; and he obtained a centre or zero of oscillation by adjusting the lower centre a little out of the vertical.

Mr. Joseph Baxendell, jun., wished to acknowledge the kindness of Mr. George Knott, F.R.A.S., in furnishing him with the dates of 11 maxima and 9 minima of the variable star R. Ursæ Majoris, from which he deduced a difference between the calculated and observed times of maxima of 12·46 days, the greatest difference being 24·92 days, and of the minima 8·17 and 21·92 days respectively.

In a paper on the variable star χ Cygni Mr. J. E. Gore, F.R.A.S., said this interesting variable had lately passed through a maximum of about average brightness. His observations had fixed the date as Nov. 23rd, 1884, when he thought the star was perceptibly brighter than δ Cygni. The average period seemed to be about 406·5 days, but, according to Schönfeld, the period was slightly lengthening. The observations had been made with a powerful binocular, but the star was above 6 mag. or visible to the naked eye from Nov. 2, 1884 to Jan. 6, 1885, or a period of 65 days.

Mr. G. T. Davis had noticed that a fifth star had lately become visible in the trapezium with his 3¾ in. Wray. This star was s. f. the brightest star, and was probably identical with Herschel's 6th star, which Webb thought would prove variable. In a description of the Alpine valley Mr. T. G. Elger, F.R.A.S., said the scenery in this neighbourhood was extremely difficult to sketch, especially under a high sun, when alone the conditions prevail which permitted the complicated details to be clearly seen and interpreted from end to end. It was only when far removed from the terminator that its V-shaped outlet to the M. Imbrium could be traced to advantage, or the flask-like expansion with the constricted gorge which led up to it from the north-west satisfactorily observed. At other times these features were always more or less concealed by the shadows of neighbouring heights. The details of the upper or more attenuated end of the valley were however seen best under a setting sun,

when many striking objects came to light, of which few traces appeared at other times.

Mr. A. Stanley Williams, F.R.A.S., called attention to a singular variation in the brightness of the lunar crater Boscovich. The crater itself was known to students as one of the darkest and most obscure objects on the moon, being only half a magnitude brighter than the black shadows. Some months ago he had however seen it distinctly brighter, but lately it had again darkened until it was almost as black as the photometric measures made it. From the length of time over which the changes extended they could not be produced by variation in the angles of illumination, but would seem to be due to some physical cause. The Secretary said that this seemed to be only another instance of the want of a comparatively cheap and reliable map of the moon, and he was glad to say the publication of such a map was now in contemplation by the Society. It had at first been suggested that the map should include all detail observable with a 9in. aperture, and probably they might attempt this later on, but he thought at present this would be too expensive an undertaking. Probably a map about 3ft. in diameter and with markings within the scope of about 4-inch telescopes would be most suitable. The matter was however still in abeyance, and he would be glad to receive suggestions.

Mr. W. F. Denning, F.R.A.S., thought the idea a good one, and he hoped it would not fall through, as the Society numbered many who had given especial attention to selenography. He was of opinion that something on a less extensive scale than was originally suggested might be attempted.

The President said that a 9-inch reflecting telescope had generously been placed at their disposal by Mr. George Calver, F.R.A.S. It was already at work on Birmingham's red stars, and he hoped shortly to give a favourable account of its performance.

Nine gentlemen were elected members of the Society.

SELENOGRAPHICAL NOTES, MARCH, 1885.

By Thos. Gwyn Elger, F.R.A.S.

The Great Alpine Valley. As supplementary to the notice of this singular formation which appeared in September last, it may be of interest to refer to observations made subsequently when the whole of the valley was scrutinized under equally favourable though different conditions. All who have made the attempt are aware that, except when the valley is near the

terminator it is very difficult to make out its more delicate details. At other times, even with steady definition, its outline usually presents a more or less indistinct aspect, which no adjustment of the eye-piece will remove. This is, to a great extent, the case with all lunar objects when viewed under a high light, but is especially so in this particular instance mainly because the formation is simply a vast depression, devoid, except near its south-eastern end, of lofty and bright borders to mark out its limits. Consequently, although the details of either end may often, when near the terminator, be observed satisfactorily, it is only on rare occasions that the valley can be well seen as a whole, a condition which is absolutely necessary in order to appreciate its true character. At 19h. on 25th January, the E. long. of the morning terminator was about 29°, hence the sun was at a sufficient altitude to show the complicated mountain-system which bounds the northern shore of the Mare Imbrium without the intervention of shadows pronounced enough to obscure and confound its details. The curious V-shaped pass, bounded on either side by lofty Alps, which connects the Mare Imbrium with the large expansion of the valley near its lower or south-eastern end, was a conspicuous and interesting feature, as was also this expansion itself, which, though invariably represented as oval in shape, resembled rather a parallelogram with one corner (the southern) rounded off. As regards the form of the wedge-shaped portion of the valley extending towards the Mare Frigoris, its south-western margin is almost perfectly straight up to a point nearly opposite the outlying crater D (*see Neison's Map VI.*), where there is a slight inward bulge. Beyond this, in common with the whole of the extreme end of the formation, it curves very gently towards the north. The opposite border is extremely irregular, so much so, that there is scarcely a straight piece in it. Occurring at intervals on the floor of the valley under the south-western border, six well-defined black spots were remarked. Two of them are evidently craterlets with bright rims, and the remainder are probably of a similar character as they are evidently not shadows of peaks on the wall, or they would be still more conspicuous under a lower angle of illumination, which is not the case. The objects seen by Schroeter and his assistant, Harding, on 8th November, 1796, and represented in *Sel. Top. Frag.* T. LXX., Fig. 36, are clearly of the same nature, if not absolutely identical. On the same evening Schroeter saw a small crater-like spot on the opposite slope of the valley, which has not, I believe, been observed since. He places it about the point where the cleft which crosses the valley intersects the border. In the Rev.

Prebendary Webb's excellent sketch of the formation (*Intellectual Observer, Vol. IX. p.* 174), a narrow ridge or landslip is represented as extending from the mountain bounding the southern side of the neck of the flask-shaped expansion for some distance along the centre of the valley. This feature, which is faintly indicated in Schroeter's drawing, was easily traced. On 23rd and 24th January the cleft just alluded to was very distinct. It was followed from a large isolated mountain, situated some miles on the southern side of the valley, across the floor to a point about the same distance on the northern side. It makes a visible gap in the northern border, and traverses the interior somewhat obliquely, inclining slightly towards the upper end of the valley. On the following night, January 25th, though under a higher angle of illumination, it was still a prominent object, and a number of craterlets associated with it were seen in addition, one of which, situated near the place where the cleft cuts through the northern border, was very bright and conspicuous, and possibly answers to the small circular spot shown in Schroeter's drawing, though this is represented as situated on the inner slope of the valley. Abutting on the cleft on the south-western side of the valley are three parallel rows of low ridges or hills, one of which extends in a south-easterly direction for a distance of sixty miles or more in a nearly straight line. The region north of the valley through which the cleft passes also displays many curious examples of parallelism in its minor details. There are numbers of little hillocks and ridges all trending in a direction corresponding to that of the axis of the great depression. Though there appeared to be at times indications of the extension of the cleft towards the north, it could not positively be traced beyond a craterlet north of the mountain mass η (*Neison, Map VI.*) It probably, however, joins the long cleft which runs eastwards from the upper end of the valley to which reference was made in September last. At or near full moon the cleft can often be traced as a faint white line crossing the valley. I saw it thus, with an aperture of 4 inches, on 11th December, 1872, without, however, suspecting its true nature. As a known object, it can be made out in Rutherfurd's and other photograms. Its direction beyond the lofty isolated mountain on the south side of the valley is continued by a row of hills which extends in a bold curve for a considerable distance, a circumstance of such frequent occurrence that it is probably due to some as yet unexplained physical connection between clefts and the ranges of hillocks which often form apparent prolongations of their course.

Kempston, Beds: February 16, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

AEROLITIC FIREBALL.

Sir,—What appears to have been a magnificent aerolitic fireball is described in the Isle of Wight newspapers of January 31st, 1885. The following accounts appeared in the *Isle of Wight Journal and Newport Times*:—

"DESCENT OF A FIREBALL.—Great alarm was caused early on Sunday morning [January 25th] at Guernsey by a terrific explosion heard all over the island and far out at sea. Some thought it was an earthquake, others supposed it unusually heavy thunder. The crew of the steamer 'Commerce,' corroborated afterwards by others, spoke to having seen, about three o'clock, when nearing Guernsey, an enormous ball of fire shoot from the heavens and fall into the sea, to the west of the island, the fall being attended by a terrific explosion. Houses vibrated with the shock, and some persons were so frightened as to be deprived of their senses."

"EMSWORTH (Isle of Wight).—A meteor of unusual brilliancy burst in a south-westerly direction about 2.50 a.m. on Saturday morning [January 24th]. The Chichester mail contractor, who was driving towards this town when it made its appearance, describes it as a huge blaze of light, which powerfully illuminated the whole country round. Everything was for a time as visible as in broad daylight. P.C. Gladwell, who was on duty at the time, states that the phenomenon was one of startling grandeur. For the instant he was impressed with the idea that the whole of Emsworth was in flames."

The close agreement in the hour and position of this vivid detonating meteor, as described at Guernsey and at Emsworth, Isle of Wight, prove them to refer to one and the same object, though there is an error of date in one of the two accounts. It will be a matter for regret if these imperfect records are the only ones preserved of a meteor which appears to have been remarkable, not only on account of its exceptional splendour, but also for the loud detonation which closely accompanied its disruption.

Yours faithfully,

Bristol: Feb. 16, 1885. W. F. DENNING.

ASTRONOMICAL OCCURRENCES FOR MARCH, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Sun	1		Sidereal Time at Mean Noon 22h. 37m. 49·10s.			11 28·6
Mon	2		Sun's Meridian Passage 12m. 15·41s. after Mean Noon	2nd Tr. E. 2nd Sh. E. 3rd Oc. D.	5 53 6 27 14 6	11 24·2
Tues	3			1st Oc. D.	16 16	11 19·8
Wed	4			1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	13 34 13 54 15 54 16 14	11 15·4
Thur	5			1st Oc. D. 1st Ec. R. 2nd Tr. I. 2nd Sh. I.	10 42 13 20 29 16 5 16 48	11 11·0
Fri	6	12 52 14 2	Occultation of θ Libræ (4½) Reappearance of ditto	3rd Tr. E. 1st Tr. I. 1st Sh. I. 3rd Sh. E. 1st Tr. E. 1st Sh. E.	7 19 8 0 8 23 8 50 10 20 10 42	11 6·6
Sat	7			1st Ec. R. 2nd Oc. D. 2nd Ec. R.	7 49 4 11 4 14 46 27	11 2·2
Sun	8	6 54 14	☾ Moon's Last Quarter Neptune at quadrature with the Sun			10 57·8
Mon	9	16 25 17 38 17 6 18 26	Occultation of B.A.C. 6287 (6) Reappearance of ditto Occultation of B.A.C. 6292 (6) Reappearance of ditto Saturn's Ring: Major axis=41″·68 Minor axis=18″·90	2nd Tr. E. 2nd Sh. E.	8 8 9 2	10 53·4
Tues	10	17 18 18 38	Occultation of ρ^1 Sagittarii (4) Reappearance of ditto	1st Oc. D.	18 0	10 49·1
Wed	11	0 12	Conjunction of Mars and Sun Conjunction of Venus and Mercury 0° 44′ S.	1st Tr. I. 1st Sh. I.	15 19 15 49	10 44·7
Thur	12	22 23	Conjunction of Moon and Venus 5° 9′ S. Conjunction of Moon and Mercury 5° 58′ S.	1st Oc. D. 1st Ec. R.	12 27 15 14 43	10 40·3
Fri	13			3rd Tr. I. 3rd Sh. I. 1st Tr. I. 1st Sh. I.	7 1 9 11 9 45 10 17	10 36·0

DATE.		h. m.	Principal Occurrences.	Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m. Jupiter.
Fri	13		[13*th day continued.*]	3rd Tr. E. 1st Tr. E. 1st Sh. E. 3rd Sh. E.	10 39 12 5 12 37 12 49	
Sat	14	16	Conjunction of Moon and Mars 4° 30′ S.	1st Oc. D. 1st Ec. R. 2nd Oc. D.	6 53 9 43 20 13 21	10 31·6
Sun	15		Illuminated portion of disc of Venus= 0·976 Illuminated portion of disc of Mars=0·996	1st Sh. E.	7 6	10 27·3
Mon	16	5 36	● New Moon Sidereal Time at Mean Noon 23h. 36m. 57·37s.	2nd Tr. I. 2nd Sh. I. 4th Sh. E. 2nd Tr. E. 2nd Sh. E.	7 28 8 41 10 14 10 24 11 36	10 23·0
Tues	17		Sun's Meridian Passage 8m. 23·46s. after Mean Noon			10 18.6
Wed	18	6 58 7 54 20	Occultation of B.A.C. 481 (6½) Reappearance of ditto Opposition of Jupiter to the Sun	2nd Ec. R. 1st Tr. I.	6 40 36 17 4	10 14·3
Thur	19			1st Oc. D. 1st Ec. R.	14 12 17 9 5	10 10·0
Fri	20			3rd Tr. I. 1st Tr. I. 1st Sh. I. 3rd Sh. I. 1st Tr. E. 3rd Sh. E. 1st Sh. E.	10 24 11 31 12 12 13 10 13 51 14 2 14 32	10 5·7
Sat	21	6 30 6 58 8 37 9 34 10 0 6 52 7 54	Occultation of B.A.C. 1351 (6½) Reappearance of ditto Occultation of 75 Tauri (6) Reappearance of ditto Near approach of B.A.C. 1391 (5) Occultation of α Tauri (1) Reappearance of ditto	1st Oc. D. 1st Ec. R. 1st Oc. D.	8 38 11 37 45 15 39	10 1·4
Sun	22	6 52 7 54 9 4 9 23 20	Occultation of 111 Tauri (5½) Reappearance of ditto Occultation of 117 Tauri (6) Reappearance of ditto Conjunction of Moon and Saturn 3° 44′ N.	1st Tr. E. 1st Sh. E.	8 17 9 0	9 57·2
Mon	23	5 23	☽ Moon's First Quarter	2nd Tr. I. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	9 46 11 16 12 41 14 12	9 52·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Tues	24	11 39 12 37	Occultation of 68 Geminorum (5½) Reappearance of ditto	4th Oc. D. 4th Oc. R.	8 49 13 29	9 48·6
Wed	25	12 41 13 38	Occultation of B.A.C. 2872 (6) Reappearance of ditto	2nd Ec. R.	9 16 56	9 44·4
Thur	26	13 45 14 36	Occultation of *h* Leonis (6) Reappearance of ditto	1st Oc. D.	15 58	9 40·1
Fri	27	10 24 11 23 11 37 12 28	Occultation of B.A.C. 3529 (6) Reappearance of ditto Occultation of 43 Leonis (6) Reappearance of ditto	1st Tr. I. 3rd Tr. I. 1st Sh. I. 1st Tr. E.	13 18 13 50 14 7 15 37	9 35·9
Sat	28	10 19 11 19 12 14 13 25 13 36 14 17 15 57 16 54 0	Occultation of B.A.C. 3836 (6) Reappearance of ditto Occultation of 75 Leonis (5½) Reappearance of ditto Occultation of 76 Leonis (6) Reappearance of ditto Occultation of 79 Leonis (5½) Reappearance of ditto Conjunction of Moon and Jupiter 4° 27′ N.	1st Oc. D. 1st. Ec. R.	10 25 13 32 17	9 31·7
Sun	29		Saturn's Ring: Major axis=40″·22 Minor axis=18″·29	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	7 44 8 35 10 4 10 55	9 27·5
Mon	30	4 40	○ Full Moon	1st Ec. R. 2nd Tr. I. 2nd Sh. I. 2nd Tr. E.	8 0 54 12 6 13 51 15 1	9 23·3
Tues	31	9 20 10 26	Occultation of B.A.C. 4591 (6) Reappearance of ditto	3rd Ec. R.	10 41 50	9 19·1

OCCULTATION OF ALDEBARAN.

The re-appearance of Aldebaran on the 22nd, was well seen here, 5h. 42·4m. G.M.T., correct probably to the nearest tenth of a minute. The ruddy star very bright on the illumined limb of the moon, not far from the terminator, was a pretty sight. I missed the disappearance.

Teignmouth, Devon: G. J. W.
February 24, 1885.

LONGITUDE of MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator. E., Evening Terminator.

1885.	° ′		° ′		° ′
Mar. 1	+84 18 E.	Mar. 11	−37 28 E.	Mar. 21	+20 43 M.
2	72 7	12	49 40	22	8 32
3	59 57	13	61 50	23	− 3 40
4	47 46	14	74 1	24	15 50
5	35 36	15	86 11	25	28 2
6	23 25	16	+81 37 M.	26	40 13
7	11 14	17	69 26	27	52 24
8	— 0 56	18	57 16	28	64 35
9	13 7	19	45 5	29	76 47
10	25 17	20	32 54	30	88 58
				31	+78 50 E.

Moon farthest from the Earth, Mar. 9, 9h.
,, nearest to ,, ,, ,, 23, 9h.

THE PLANETS FOR MARCH.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	22 20 39	S. 12 37½	4″·8	23 38·9
	9th	23 15 10	S. 6 46½	4″·8	* * *
	17th	0 4 31	S. 0 38½	5″·0	0 23.6
	25th	1 1 18	S. 6 54	5″·6	0 48·7
Venus ...	1st	21 54 31	S. 13 55½	10″·2	23 12·9
	9th	22 32 52	S. 10 33	10″·0	23 19·7
	17th	23 10 17	S. 6 52	10″·0	23 25·5
	25th	23 47 0	S. 3 0	9″·8	23 30·7
Jupiter ...	1st	10 8 16	N. 12 50½	41″·8	11 33·0
	9th	10 4 33	N. 13 10½	41″·4	10 53·4
	17th	10 1 13	N. 13 28½	40″·9	10 18·6
	25th	9 58 25	N. 13 43	40″·3	9 44·4
Saturn ...	1st	5 5 55	N. 21 38	16″·9	6 27·0
	9th	5 6 58	N. 21 41	16″·6	5 55·6
	17th	5 8 31	N. 21 45	16″·4	5 26·7
	25th	5 10 31	N. 21 49	16″·1	4 57·3
Uranus ...	2nd	12 7 23	N. 0 3	4″·0	13 23·5
	18th	12 4 55	N. 0 12	4″·0	12 18·1

Mercury is very close to the sun at the beginning of the month. On the 21st, he sets about 40 minutes after the sun, the interval rapidly increasing.

Venus is badly situated for observation, rising about half an hour before the sun at the beginning of the month, the interval decreasing. Towards the end of the month she rises a little before the sun.

Jupiter sets just before sunrise on the 1st, and afterwards earlier each night.

Saturn sets two hours and a half after midnight at the beginning of the month, the interval decreasing.

Uranus is well situated for observation.

Books received.—Nineteenth Report of the Board of Visitors to the Observatory at Victoria. 1884.—Boletin de la Institucion Libre de Ensenanza. No. 192.—Sur L'Aggrandissement apparent des Constellations, &c. Par M. Paul Stroobant. Bruxelles. F. Hayez. 1885.—The Summary of a Meteorological Journal. By C. L. Prince.—Bulletin Astromique. By Tisserand.—L'Astronomie. By Flammarion.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1884.

Bridson, J.
Dale, R.
De la Rue, W.
Lean, W. S.
Monk, Dr.
Pritchard, Rev. Prof.
Smith, Dr. R.
Waldegrave, Hon. H. N.

To June, 1885.

Grove, S.

To Dec., 1885.

Aldam, W.
Cooke, J. S.
Draper, Mrs.
Herschel, Professor
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TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to JOHN C. JACKSON, Chief Office, London.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 268. APRIL. 1885.

ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

The first meeting of the new session was held at the Society's Rooms, Burlington House, on Friday, March 13th.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut-Col. Tupman.

The minutes of the January meeting were read and confirmed.

The following gentlemen were balloted for and duly elected Fellows of the Society:

Professor W. Steadman Aldis, M.A., University College, Auckland, New Zealand.

Lieut. Henry Edward O'Neill, R.N., Mozambique.

William Peck, 6, Hanover Street, Edinburgh.

Col. Tupman announced that 91 presents had been received since the last meeting; among them were three photographs of stars taken by Mr. Common with a small lens, about 4½-inches in diameter. One of the photographic plates had had 20 minutes exposure, another one hour. One of the plates showed stars down to the 9th magnitude.

General Tennant mentioned that one of the photographs, that of the region about Aldeberan, showed 1,800 separate stars which had been identified. That was an hour's work.

Mr. Lecky, who was called upon by the President, said: The Society will recollect that a few months ago a statement was made here publicly to the effect that the celebrated manufactory

of optical crown glass at Paris was untrustworthy. At the beginning of this year, I got a letter from M. Chas. Feil in which he requested me to state that he had rejoined the concern, and had taken into partnership M. Mautois. I wrote to the Editor of *Nature*, and he kindly put in a notice stating that M. Feil had started business again.

Lord Crawford: I am glad to hear there is a chance of good optical glass being again obtainable. Last time I attempted to procure any it was so bad that whereas I wanted a prism 6-inches square I was not able to get more than 2-inches. The manufacture of crown glass seems to have gone thoroughly out of knowledge.

General Tennant: A very remarkable example of glass making is mentioned in the *Journal of the Society of Arts*, by one of the Siemens's. I understand he is able to mould glass at something like 5s. or 5s. 6d. per cwt., to any form you like. Mr. Common has, I think, told me that for the glass for his 5-foot mirror he paid £300, and that to-day he could get it for £25.

Mr. Hilger mentioned that he had obtained glass 4½-inches square which left nothing to be desired. There was no polarisation, but there was no deficiency as regards definition under high magnifying powers.

Mr. Ranyard: Is that Chance's glass?

Mr. Hilger: Yes; but I get glass from Paris which is equally good as regards density.

Mr. Downing read a paper on *The Star Places of the Nautical Almanac*. In the *Nautical Almanac* for 1884, he said, a change was introduced; the mean places of 182 stars having been derived from the Greenwich 9-year catalogue. The places in the 1883 *Nautical Almanac* were taken from the Greenwich 7-year catalogue. In looking over the discordances between the *Nautical Almanacs* for 1883 and 1884, and confining his attention to the R. A. of clock stars as being the most important, five hundredths of a second seemed to be an abnormal discrepancy. There were 24 stars with a discrepancy of more than five-hundredths of a second. The average discordance for the 24 stars was s·064. These discordances appeared to arise from inaccuracies in the proper motions which had been adopted. In the 1883 *Nautical Almanac* there would be about 22 years' proper motion, and in bringing up the stars to the epoch 1884 from the 9-year catalogue, there would be 12 years, so that there would be a difference of 10 years proper motion. The proper motions which had been adopted had been taken from the lists computed by Main and Stone, by comparing Airy's earlier Greenwich catalogues with Bessel's reduction of Bradley's observations. He had substituted

the proper motions deduced by Auwers from the new reduction of Bradley compared with modern observations, and the average discordance had been diminished from s·064 to s·042. That appeared to demonstrate the superiority of Auwers' proper motions, and showed the desirability of their adoption in this country. He had also given in this paper the systematic corrections necessary to reduce the star places of the *Berliner Jahrbuch*, the *American Ephemeris*, and the *Connaissance des Temps* to the system of the *Nautical Almanac* for 1884, making use of the comparisons which Auwers had made between these authorities and the *Nautical Almanac* for 1883. The great majority of large discordances which Auwers had found would have disappeared if he had used the *Nautical Almanac* for 1884 instead of that for 1883.

Mr. Knobel: This is a very important paper, and I think it is extremely satisfactory that Mr. Downing has now shown that the *nine years* catalogue adopted in the *Nautical Almanac* places are so very superior. We may be very glad indeed that Mr. Downing has brought this forward.

Mr. Downing read a second paper on the *R. A.'s of the Cape Catalogues for* 1850 *and* 1880. He said: Mr. Gill, in the introduction to the Cape Catalogue for 1850, had made a comparison of the R. A.'s with those of the catalogue for 1880, and found large discordances depending on north polar distance. He seemed to think that the discordances arose from errors in the right ascensions in the 1880 Catalogue, arising from the fact that the collimation was subject to variations at different altitudes. Mr. Stone, he believed, had expressed the opinion that the discordances arose from the errors in the proper motions used in bringing up the places for some 25 or 30 years. Without wishing to enter into a controversy on the subject, he desired to lay before the Society some evidence which appeared to throw light on the matter. Taking the comparison between the right ascensions of the Cape Catalogues of 1880 and 1850, there was a considerable discordance depending on north polar distance, ranging from —s·013 at 95° to —s·161 at 140°. First of all he had compared the right ascensions of the 1850 Catalogue and the 1840 Catalogue; also the right ascensions of the 1880 Catalogue with those of the Melbourne Catalogue for 1870, and with those of the Cape Catalogue for 1860. By that means he was able to compare the right ascensions of the 1850 Catalogue and of the 1880 Catalogue respectively with the mean of the three—the Cape Catalogues for 1840 and 1860 and the Melbourne Catalogue for 1870. In the 1880 Catalogue the discordance, depending on north polar distance, at 120° was —s·041, and at 150° + s·030,

being a range of s·071, when compared with the mean of the three. The 1850 Catalogue, compared with the mean of the three, gave at 100° a discordance of — s·042, and at 140° + s·138, showing that the right ascensions of the 1880 Catalogue were superior to those of the 1850 Catalogue. These comparisons might be relatively affected by discordances arising from erroneous proper motions, but to obviate that he had taken a comparison of each of the Cape Catalogues with the mean of the two—viz., the Cape Catalogues for 1840 and 1860. The comparison of the 1880 Catalogue at 120° was — s·044, and at 150° + s·048, compared with the mean of the Catalogues for 1840 and 1860. The 1850 Catalogue, compared with the same mean, gave at 100° — s·006, and at 140° + s·143. It appeared, therefore, that the discordances between the 1850 and 1880 Catalogues were more probably due to errors in the former than in the latter.

Mr. Stone: I am glad to find that Mr. Downing has been putting these numbers together, because I have only one object in view. The observations I made at the Cape were made with what I believe to be an efficient instrument, and they were made with extreme care, and were exceedingly good, and I am only too glad to see them subjected to fair tests. At the same time, when numbers of this kind are brought forward as real discrepancies, got by comparing the 1880 and 1850 catalogues, all I can say is that it is a most unreliable test. Observations about 95° and 118° being based on the ordinary clock-star list of the Greenwich observatory, are affected by much systematic error. Those at 140° are affected by the whole undetermined proper motions, including any correction for precession constants between 1850 and 1880, which is thirty years; and though people may assume that if you take one portion of the sky there is no tendency to *minus* proper motions, I do not think it is proved at all, and as far as my experience goes, the evidence is against it. There are certain regions where you get a decidedly distinct preponderance of negative proper motions, and therefore it is not at all certain that when you are taking up some 40 or 50 stars about 140° there may not be preponderance of proper motions in a certain direction. In a test I applied the other day I took the Melbourne observations of about the same period as my own—about 1875, where the case of proper motions scarcely came into play at all—and I showed there was no difference between the Melbourne and Cape observations to any sensible amount. When you are discussing questions of four-hundredths and five-hundredths of a second, you may call them large if you like, but they are only large in a comparative sense, and everybody knows that

there are a good many doubtful quantities of that kind floating up and down in astronomy. By adopting some observations of Bradley, reduced as they are, and comparing them with comparatively modern observations, with whatever precession constant you start with, you have proper motions that give the places, with a certain accuracy, for a certain number of years, and by taking two or three Catalogues and determining the value of these proper motions, you are able to get certain systematic corrections that make these Catalogues give wonderfully accordant proper motions; but if the precession constant were not quite right, then the proper motions would not be quite right, and directly any change is made this system goes all at once; and I am considerably in doubt whether we get any advantage from the application of these systematic corrections at all. If you adopt a set of R. A. for your clock-stars, thus brought up for many years, your right ascensions may be exceedingly good; but if you do, as is done at Greenwich, refer your stars every few years independently to epoch, you will get right ascensions that will not agree with the others. Which of these methods is better or worse I cannot say, but I prefer the Greenwich system of referring them to an epoch independently. If you take Newcomb's fundamental right ascensions, and he speaks of them, I think, as true to within 100th of a second, and if you compare them with the right ascensions adopted at Greenwich, you will find, at certain places, instead of those large differences which are supposed to prove the badness of the Cape observations, that you have discrepancies three times as large, or twice as large—eight 100ths of a second—and you will find that many of them are of a systematic character, dependent on the right ascension; consequently, anybody who adopts this system of right ascensions, and determines his places from them, and anybody who adopts the Greenwich system, will have systematic differences between these results, and this does not prove the badness or goodness of the work, but that there are these kinds of differences running through our fundamental right ascensions. Anybody would rejoice to see these kinds of quantities blotted out; but as long as they exist I don't think there is much use in talking of catalogues being bad, if they differ only in quantities of that order. As far as the Cape Catalogue of 1850 is concerned, I know as much as most people about it, for it was very nearly complete when I left the Cape. The observations are, I believe, fairly good. I do not believe that the instruments with which the places of the 1850 Catalogue were fixed were at all equal to the transit circle employed to obtain the places of the 1880 Catalogue.

Mr. Knobel read a paper, by Professor Oppolzer, on the proposed change in the astronomical day, advocating the adoption of universal time, reckoning from midnight; and pointing out that little difficulty would be experienced in changing the headings of the columns of the *Nautical Almanac.*

The Astronomer Royal: With reference to this paper of Professor Oppolzer, I may say I have since received a letter from him in which he says he is going to give practical effect to his views by adopting the new reckoning of time in a publication he is now preparing, comprising a list of 8,000 solar and 5,200 lunar eclipses, between 1,207 B.C., and 2,061 A.D., so that will give an opportunity of seeing whether all the inconveniences resulting from the change are really so great as some astronomers suggest. I must say I agree with his opinion, that we are inclined to over-estimate the inconvenience and trouble caused by a change of of this sort, rather than underestimate it. I have been rather surprised myself at the ease with which the change has been made at Greenwich. We have practically adopted the new reckoning for all uses there without any trouble, and for certain purposes we keep up the old reckoning for comparison with the *Nautical Almanac*, and the change is so easy that we can at any time adopt the old or the new in the published observations. I think, when the change can be so easily made as that, it is not a matter of such serious inconvenience as we have been led to infer. I am also glad to say I have received a letter from M. Struve, director of the Pulkova Observatory, saying that he intends to adopt the new reckoning of time there for internal use. The only thing he thinks requires consideration is the epoch at which the change should be introduced into the published observations; and he is inclined to recommend that that should be deferred till the new time can be adopted in the *Nautical Almanac.* That would be in the year 1890, the first year in which it can be conveniently adopted for that purpose. I think it is very satisfactory to find that Professor Oppolzer has expressed his opinion so strongly, because at the Washington Conference there was no Scientific Austrian Delegate present, and the Austrian Delegate present abstained from voting. Professor Oppolzer speaks with authority as the scientific representative of Austria. I think this paper is, therefore, of very great importance as bearing on the question.

Mr. Stone: The idea of Professor Struve is that some convenient time should be fixed, and I think that is essential. Of course it is easy enough to compute your time as at present and throw away twelve hours—there is no great difficulty in that—but we must recollect if we are making a change those who come after us will have, in all comparisons between past and

future observations, to make an allowance for this loss of twelve hours, and therefore the period at which the change is made must be one which is generally convenient, and must be marked in the most emphatic manner in our power. It will certainly be a great inconvenience to our successors, and I think we should reduce that inconvenience to a minimum. I shall be glad to see this change made. We shall see that the moment anyone makes it they will have to do with the direction of the meridian at mean noon, and that they cannot play fast and loose with the meridian at o hours, as it has always been assumed in this room that they can; and that the true unit of time is the sidereal day. If you could thus change the relative positions of the sun and meridian without error, then, as the proposed change of twelve hours would only alter the sun's right ascension by less than two minutes, the mean times determined in the usual way from the sidereal times at 0^h

$$= \frac{\text{sun's mean longitude} + \text{nutation}}{15},$$

would after the change differ by less than two minutes from the times thus determined before the change, instead of twelve hours as they should do, and your lunar observations and existing theories would entirely disagree. But if the proper correction

$$24^h . \frac{\text{change of sun's mean longitude}}{\text{sun's mean motion in one day}}$$

be added to the sidereal time, then in all cases the results will be found to be correct, whether the change of sun's mean longitude be that due to a change of time of twelve hours, one hour, a second, or the hundredth part of a second. Since 1863 we have been making small changes of this kind every day without a proper allowance for the change of sidereal time in the interval.

General Tennant: I am fully with my friend, Mr. Stone, as to sidereal time, but I want to point out that as to mean time the real difficulty seems to come to this, that Greenwich mean time is a phrase in the English language which means something definite, and it has its origin at noon, and we must call the day that begins at midnight universal time. If we do not take a new name for it we shall have endless confusion. If we recognise the difference we shall very soon find things will settle themselves down.

Mr. Stone: But it is not desirable they should settle themselves down until a point of that kind is settled; I should say that for astronomy there is only one natural unit of time, and

that is sidereal time. I intend shortly to bring out a small treatise showing that that is the natural unit of astronomy, and with it you are free from these changes or units. The difficulty that comes in with mean solar time is that if you introduce any discontinuity whatever in your mean time which separates it from sidereal time, either you have no physical basis for your mean time, or you are inevitably driven to the introduction of the term $24^h + \frac{nt}{n}$ to remove discontinuity.

The Astronomer Royal: I quite agree with Mr. Stone that the natural unit of astronomy is the sidereal day, but, unfortunately, the natural unit of astronomers is the solar day, because they have to get up by it and go to bed by it; so for these purposes we have to consider the astronomical day to mean the solar day; it is on this ground the change is brought forward. It has no connection with the sidereal day.

Capt. Wharton, R.N.: I wish to remind the Society there is another class of people who are very considerably affected by this besides astronomers, and that is sailors. If any change is made in the time, it will, unless it is done after a very considerable amount of notice being given, cause disaster, because, although an educated sailor will have no difficulty in adding his twelve hours on or taking them off, there are a great many ships, I am sorry to say, belonging to the English nation which are navigated by rule of thumb. A man learns his work of reckoning when he is young, and when he brings out his observations on the chronometer at 21 hours he knows it is 9 o'clock in the morning, and I am sure there will be great difficulty in knocking it into the heads of those masters that 21 hours mean 9 o'clock at night. Sailors are the people who the governments will have to consider. I am a sailor myself, and perhaps speak with a bias, but I think that is the view the governments of the world will take in making any formal change. The proposed change has been initiated by a formal conference of delegates from all nations of the world, and I do not think it is for any society or any individual to attempt to push the change forward before the time is ripe. I am in favour of the change myself. If you can get one day of 24 hours, instead of two distinct days, it will be a great improvement, but my own opinion is, considering that a great many of the delegates at Washington abstained from voting on some of these points, it will only tend to throw things back if one society in the nation that will gain most by the change attempts to force it down the throats of foreigners who are opposed to it. I myself would counsel a little delay.

The President: I am sure you will return your thanks to

Professor Oppolzer for his paper, and, if for nothing else, for the very interesting discussion we have had on the subject.

Mr. Spitta described a note he had presented in reference to a Transit of the 4th Satellite of Jupiter. It appeared black in colour and oval in shape, and its longer axis was parallel to the belts.*

Mr. Knobel: I observed the entrance of this satellite on the occasion Mr. Spitta mentions, but it was between intervals of clouds; and I certainly remarked one point that was curious, when first seen within the disc, it appeared as a bright spot, and five minutes after as a very small dark spot, and in about a quarter-of-an-hour it seemed a perfectly round black spot.

Mr. Spitta: I did not observe at egress at all.

Mr. Knobel: I was watching the transit of the shadow of the first satellite two hours before, and was particularly struck how round the fourth satellite appeared to be.

Mr. Spitta: It always appeared to me before to be a round spot. There are great discrepancies in the papers published about this. I have looked over forty or fifty accounts of black satellites. On some occasions observers have reported that the satellite has been round; others have seen it as oval. Others again have published diagrams giving it an irregular shape, even semi-triangular. Whether it was an optical effect or not I do not know.

Mr. Ranyard: Have you an astigmatic eye, with a vertical axis?

Mr. Spitta: No; I have examined my eyes, but there is no astigmatism.

Mr. Common: What was the observation made with?

Mr. Spitta: It was made with a 10-inch reflector.

Mr. Common: The reflector is an instrument that is rather subject of errors in adjustment, and it has a tendency to make points elliptical when it is out of adjustment.

Mr. Spitta: I have tested the instrument, and have not found elongation in other points of light.

Mr. Common: Was Mr. Knobel's instrument also a reflector?

Mr. Knobel: Yes.

Mr. Common: I should not think such an observation as that would be sufficient to justify the statement that the satellite was elliptical.

Mr. Spitta: I said it "appeared" to be. I was cautious in the statement.

The Earl of Crawford: I was in hopes that there might have

* [Note by Mr. Spitta].—I did not read my paper fully. In it I pointed out that the satellite was round at 9·45, and oval at 10·20.

been a communication from Dr. Copeland which I could have read at the meeting, describing an observation of a transit of one of the satellites, made so near to the time of opposition that Dr. Copeland was able to observe the satellite occult its own shadow on the body of the planet. It was a very curious observation, as it showed that the sun, the earth, the satellite, and the part of Jupiter's disc occulted, must have been all in one straight line at the time, and as seen from the disc of Jupiter, the earth must have appeared to be transitting the disc of the sun.

Mr. Knobel read a note from Captain W. Noble on the *Nautical Almanac* dimensions of the Saturnian system. The note was accompanied by some drawings illustrating the fact that the ball of the planet appears larger than it would appear according to the dimensions given in the *Nautical Almanac.*

Mr. Green: With regard to the drawings of Saturn I noticed a discrepancy last year. I have been trying again this year, and was obliged to make a slight allowance in the *Nautical Almanac* proportions in order to bring the ball up to Cassini's division in the rings.

Mr. Stone: I don't see Mr. Hind here, but I think when the matter was brought before the Council he said they were making some changes in the elements adopted by the *Nautical Almanac,* so that I think they are aware that the theoretical elements are not perfect.

Mr. Common: There is no doubt that they are not.

The Astronomer-Royal said: This is a paper on the spectroscopic result of the motion of stars in the line of sight in the year 1884 at Greenwich. These observations were made by Mr. Maunder and Mr. Nash as usual with the spectroscope, and it is needless to say very much about them. They were carried on in the usual way. There are one or two points of interest I may mention. There is an unusually large number of stars—namely, 54 stars which have been observed, each on an average of six nights, besides that, the spectrum of moon, sky, Venus, Mars, and two limbs of Jupiter have been observed and checked with the observations of displacement. The displacement in the case of Venus does not agree very well, it is considerably larger than it should be, that for Mars is not so far out. They are both in the right direction. The displacement agrees very well with the computation for the two limbs of Jupiter. Of course it must be understood that these measures are subject to very large errors of observation, and discordances which may fall either one way or the other, and we think by continuing them carefully we may accumulate sufficient material to determine mean results which will be fairly trustworthy. There is one particular case, and

that is, the spectrum of Sirius. Mr. Maunder has observed it on 13 nights, and made 45 measures, giving the mean motion of approach as 22 miles per second, which is in the same direction noticed of late years, and opposite to that which was observed by Dr. Huggins, and in the earlier Greenwich results; so that now we have got a progressive change of motion from 25 miles per second of recession to 20 miles or so of approach—a very extraordinary thing, and I cannot offer any explanation of it at present. I think I might add a word about this. These observations of Sirius we have been careful to have made at different times of the year, because in determining the motion of stars in the line of sight by the spectroscope it is conceivable there might be a very rapid motion of comparatively short period. We might have a motion of rotation or revolution of a body close to Sirius in the course of a few months, which would be quite inappreciable by the ordinary method of observation, but which would be detected by the spectroscope. Therefore, observations should be made throughout the year as far as may be. We have made them in the months of January, February, March, April, and December, and the results hitherto are fairly accordant, so that it is evident that there is no such change of very short period.

Mr. Maunder: There is a point which may be of interest that has been suggested to me by some of my recent observations with the spectroscope, and that is that the character of the lines of one or two stars seem to have undergone some slight change. For instance, during the years of 1875-6-7, I found the F line in the spectrum of Altair very difficult to observe, but latterly the measures have been much more accordant, and I have felt much more satisfied with them; and again there were one or two stars the lines in which I had very little difficulty in observing some years ago, which seem decidedly more difficult now. Something of the same sort has been suggested with regard to the lines of the solar spectrum. Mr. Lockyer pointed out some years ago that there was evidence of the magnesium lines changing from time to time from maximum activity to that of minimum, and undoubtedly the lines in spot spectra do change their character at different times of the sun's activity, so that it is possible that similar changes in the character of the lines may also occur in the spectra of some of the stars. I can hardly say that our observations are yet sufficient to do more than suggest the possibility of such a change, but at any rate I thought I would mention my suspicion.

Mr. Ranyard: The stellar spectrum is an integration spectrum from the whole surface of the star. It would be a much more

remarkable change than the change which takes place over any limited area as in the chromosphere.

Mr. Maunder: Mr. Lockyer suggested that the magnesium lines differ, that is, the character of the lines in the general spectrum of the sun. You will find it in his *Solar Physics*. The idea is based upon drawings not upon photographs. Photographic records would be more satisfactory as a basis for such a suggestion.

Mr. Ranyard: Changes of brightness undoubtedly occur in the chromosphere during the sun spot period, but I do not know that there is any satisfactory evidence that there is any change of relative intensity of the lines of the integration solar spectrum during the sun-spot period.

Mr. Downing: As to the case of Sirius it is not favourably situated for observation at Greenwich, on account of its great zenith distance. It is desirable that these observations should be made at some observatory where the star attains a greater altitude, some observatory in the South of Europe would be best —Nice perhaps best of all.

Lord Crawford: I should like to know before going to ballot if there is any statement to be made by the Council with reference to a circular which I signed, and which was, I believe, signed by more than a hundred fellows of the Society.

The President: In reply to the question of Lord Crawford, I do not think I have anything to communicate to this meeting. He will find a resolution passed by the Council in the minutes, if he will refer to them. I have not been instructed to make any communication to the meeting this evening.

Lord Crawford: Then I suppose the Council will not be surprised if, in the ordinary course of events, a requisition is sent in to have a special general meeting, in order to take that matter into consideration. As I apprehend it, there is a strong feeling amongst those fellows of the Society who are non-residents in London, that they should have a voice in the proceedings of the Society, which at the present moment is denied them.

Mr. Common: If I may be allowed to interpose in the interests of order, in the first place Lord Crawford is utterly wrong in moving in a question that relates to the alteration of bye-laws at an ordinary meeting. In the second place, waiving that, I think a great deal of misunderstanding might be removed if our President were to state the conclusion which the Council have arrived at to-day. It must come out sooner or later, and there is no reason why it should not come out now.

Lord Crawford: I am perfectly willing to abide by the President's ruling, but I think the Society has a right to know what has been done.

The President: There can be no objection on the part of the Council to make public the resolution which has been passed to-day. I have sent the Assistant Secretary for it.

Mr. Beck: May I understand as a point of order, is this a question of an alteration of the bye-laws?

Mr. Common: No, sir.

The President: Yes it is.

Mr. Beck: Some one says it is not, and you, sir, in your official capacity, say it is a question of the alteration of the bye-laws. Would you allow me to enquire of you whether there is not a definite bye-law as to the way in which the bye-laws are to be treated, and as to how alterations are to be made—whether they are to be treated of by a resolution of the Council, or whether they are to be treated by motion at general meetings by the Fellows of the Society? I merely ask for information, having been informed by Lord Crawford that it is a question of the alteration of the bye-laws.

Mr. Knobel read Bye-law 45, Section 81.

The President: After reading that bye-law it would be improper to go further.

Lord Crawford: At what period of the Society's meetings may a Fellow suggest that a bye-law should be altered?

The President: You must give proper notice according to the bye-law. The bye-law says that any five Fellows by giving proper notice can call a general meeting.

Lord Crawford: I want to put myself in order. No one is more desirous of keeping to the letter of the law than I am.

Mr. Stone: As this matter has been before the Council they ought to be able to communicate the result arrived at.

The Astronomer-Royal: I must protest against a meeting for scientific purposes being converted into a meeting for the discussion of bye-laws. There is a great distinction between the two meetings, and I think we shall spoil the character of these meetings if we allow a discussion of this kind to be sprung upon us.

Mr. Beck: It will be altogether out of order for the Council to report to this meeting any result at which they have arrived with regard to the bye-laws. (Cries of "No.") As I understand it, the alteration of the bye-laws rests with a meeting of the Fellows of the Society especially convened, or general meeting specially convened by special requisition of Fellows, and that the alteration of bye-laws must be discussed at such a meeting, and that it is not competent for the Council to express an opinion upon any alteration of the bye-laws at an ordinary meeting of the Society, but only at a special meeting which has been con-

vened in accordance with the rules and regulations of the Society.

Mr. Common: I want to make one remark, and that is this, that as far as matters have gone everything has been in accordance with the bye-laws. A certain number of the Fellows of the Society have drawn the attention of the Council to a certain want—whether it be a change of the bye-laws or what you like. They have put the matter in the hands of the Council. If the opinion of the Council agrees with the wishes of those Fellows, the matter comes on in the ordinary course. If it does not agree with their wishes, five of them, if they are not satisfied with the decision of the Council, can call a special meeting; but as far as I can see now everything has been in accordance with the bye-laws, and I cannot see the slightest objection to communicate to this meeting the decision of the Council to-day. As to the application of the Fellows, it has been all made in due order. We have done our business, and it does not interfere with the business.

Lord Crawford: Mr. Common, in very able language, has stated precisely what I desire to obtain. I did not say anything about the bye-laws, but I confined myself to saying that a statement or request was placed before the Council some time previous to their last meeting. I made an enquiry of the President as to the requisition at the last meeting. I was told that the Council were going to take it into consideration and that I should probably learn the result at this meeting. I waited patiently all through the evening and I have heard nothing at all, and I am blamed by the Astronomer-Royal for rising up in my place, which is my just right, to ask whether the information is to be placed before us. We ought to know what the Council intend to do, because, if we are not informed we lose time. We lose a month by not knowing now, because I shall not be able to have the notice prepared before the next meeting, and that will involve another month before we can have a special meeting, and if it is put off to another month it is put off to the end of the session and the matter will be shelved. It is a matter of great importance, I will not say to all the Fellows, but to a certain section of the Society.

Mr. Knobel: I may possibly agree with every word that Lord Crawford has said, but I do not know how he gets over the bye-law which says that certain matters may be brought before ordinary meetings "provided always that the same do not relate to the bye-laws." The requisition which Lord Crawford refers to distinctly stated that it was a request to alter the bye-laws, and the word "bye-laws" is mentioned. The subject he brings forward does relate to the bye-laws, and how Lord Crawford can recognise that he is in order I am at a loss to understand.

Lord Crawford: Will Mr. Knobel tell me how he is going to call a special meeting of the Society?

The Astronomer Royal: By due notice given to every Fellow.

Lord Crawford: I have not asked anything about bye-laws, but about the requisition to the Council.

Mr. Knobel: That request was as to an alteration of the bye-laws.

Lord Crawford: Then I can only move at a special meeting.

Mr. Common: When any number of Fellows give notice to the Council that they desire a certain thing done, the bye-laws distinctly indicate that the Council shall give their decision to those Fellows.

A Member: Is that so?

Mr. Common: It is so. They have made their decision, why not give it?

The President: I am quite prepared to read the resolution, and I think we had better bring this discussion to a close by reading it. I may say the Council to-day spent a considerable time in discussing this question with great fairness on all sides, and they came to this conclusion, which I will read to the meeting, "That the signatories to the request to the Council to take steps for altering the bye-laws so as to permit Fellows unable to be present at the Annual Meeting to vote by proxy, be informed that the Council do not propose to recommend such a change; the Council, however, are of opinion that to enable all Fellows who cannot attend the Annual General Meeting to vote for the election of officers and Council by post under suitable precautions, might fitly form the subject for discussion at a special general meeting of the Fellows." That is the special resolution which has not been unanimously, but very nearly unanimously, passed by the Council.

Lord Crawford: Will you kindly inform me how I can put myself in order to move an amendment?

The President: By simply getting five Fellows to sign a requisition to the Council to call a special meeting for such and such a purpose, and that will be laid before the Council at the next meeting, and they will then give the usual notice, so that the proposed alterations may be considered at the May meeting.

Lord Crawford: May I ask if you will kindly lay down the law on another subject? In the event of my placing a resolution upon the table, it becomes the prime question at a meeting of that description. Will you tell me if it is competent for anybody to move even a verbal amendment to that without giving a month's notice?

The President: No; you cannot move a verbal amendment.

Lord Crawford: I think by the rules we can.

The President: I think on one occasion you ruled the contrary.

Lord Crawford: I did so following the directions of the Council. I do not of course wish to move an amendment to my own resolution, but I think the matter ought to be settled.

The meeting then adjourned.

THE LIVERPOOL ASTRONOMICAL SOCIETY.

The sixth meeting of the session was held on March 9th, the Rev. T. E. Espin, B.A., President, occupying the chair. A paper, by Dr. Klein, was read on the dark spots between Copernicus and Gambart. These spots were only visible under a high illumination, and had, up to the present time, remained an enigma to the observer. Schmidt had reported on them in his work on the moon; but he did not include them in his map, because they were of too evanescent a character, and being only visible when the sun attained a high altitude, and, of course, when the true shadows had departed. They were traversed in every direction by rays, and it was almost impossible to determine their exact positions. Schmidt had, however, always been anxious that their mystery should be solved, and he had urgently commended a close study of their appearances and positions. For a more exact knowledge of the physical nature of the moon the discovery of the true nature of these spots was of the greatest importance. Respecting his own observations, which had been already reported, he had not been able, during a considerable number of years, to perceive anything that would tend to a decisive conclusion as to the real nature of these formations; but after many vain efforts, he had been able, only last January, to fix the positions of a number of these spots, so that their identity, under high illumination, could not leave any uncertainty.

The region E. of Copernicus showed numerous dark and frequently isolated hills, from about 328 English feet in height down to the finest spots, which had the appearance, under a high power, of isolated masses of rock, and which were only visible under the most favourable circumstances. Quite different was it on the W. and brighter part. There the number of the smallest craters, whose diameter could not exceed 500ft., was quite enormous; the minute craters between Copernicus and Eratosthenes appearing of great size by comparison. On January 25, 1885, he was induced by the clearness and tranquillity of the air to scrutinise them very carefully. At this time the remarkable crater,

indicated by Schmidt as *m*, had already shown its grey mimbus very distinctly, and the whole area towards Gambart C appeared perforated, like a sieve, with the smallest craters. He had been observing a faint dark streak which appeared to be sweeping over that region from S.W. to N.E., when, at 7h. 3m. p.m., there suddenly came forth a small dark spot at the S. border. It appeared in the region where numerous craters of the very smallest kind exist, but could not be identified with one of them, and it certainly had not been visible a quarter of an hour earlier.

Mr. W. S. Franks, F.R.A.S., was glad to see that Mr. Gage was taking up star colours, which, he thought, was an interesting but neglected branch of astronomy. Ever since the time of Sir W. Herschel the colours of *double* stars had been carefully registered to the almost utter exclusion of naked eye stars which did not happen to have *comites.* The Rev. T. W. Webb was, he believed, the first to call attention to the colours of single as well as of associated stars, and it was by his advice that he had made some observations of the colours of nearly all the naked eye stars visible in these latitudes between 1877 and 1878. He intended, probably next winter, to form a new catalogue of star colours, and proposed that the objects should be simultaneously observed.

The President said Dr. Copeland, of Dun Echt, had been kind enough to call his attention to the occultation of Jupiter's II. satellite by its own shadow on the night of February 19th. The evening, which began exceedingly clear, had, however, turned foggy later on, and he missed the most important part of the observation. He presented a list of 32 red stars, which, so far as he was aware, were not included in Birmingham's or any other red star catalogue. Several of them had been discovered by photography. In the zone at present being photographed, the great nebula in Andromeda occurs. The nebula appears much more sharply defined on the *p.* side, and several dots appear on the negative *s. p.* the nucleus, which are probably additional stars; though the difficulty of determining which were stars and which were specks on the plate was very considerable, and it was quite possible that some of the stars entered in the drawing would turn out not to be stars at all. On the *s.f.* side of the nebula a star of 6·5 mag. is buried in the nebulosity, which seemed more diffused at this part, the north being very sharp. The limits of the nebula, as laid down by Argelander, and those on the negative agreed very fairly, with the exception of the *s. p.* portion, which seemed to be less in the photo. than in the telescope. The nebula was, no doubt, deficient in photographic light; whilst the Orion nebula, on the contrary, very quickly penetrated the sensitive film. The equivalent star magnitude of the centre of

the nebula varied in Pickering's photometer from 6·1 on the 7th of September last to 4·8 on the 5th November, whilst the photographic magnitudes of both these periods was 8.3.

Prof. Piazzi Smyth, Astronomer Royal for Scotland, described some interesting phenomena attending the disappearance of Aldebaran at the last occultation. At 5 p.m., the moon being high up in the sky, and with the star close outside its dark limb, formed a very pretty but faint group in the little telescope, for the sun was still shining. Unfortunately the air, although quite free from actual cloud, was so full of illuminated coal smoke as to quite conceal any trace of the dark limb whereat the star was to be presently occulted. Presently, while he was still earnestly looking, the star disappeared, so suddenly and completely that he called to his assistant "mark," with the utmost confidence, though, fortunately, without taking his eye from the telescope. Almost immediately he saw the star again for 2·6 seconds, when it disappeared once more, and this time finally. As to the heretical feature of seeing two immersions of the star, he gave it simply as it occurred; though he would have had more confidence in its being a reliable observation if the dark limb had, at the time, been visible, or if the telescope had been larger and under the protection of the dome on so gusty and inclement a night.

In submitting a voluminous report from the planetary section on Saturn, Mr. W. S. Franks said few prefatory remarks were needed. He had issued a circular last October directing the attention of the members to such salient features as might be best studied during the then approaching and unusually favourable opposition. Thirty-eight sketches of the planet were given in, accompanied by careful notes by the observers, who included the Revs. S. H. Saxby, T. Perkins, J. Bone, and Messrs. J. M. Offord, G. T. Davis, S. M. B. Gemmill, J. Ellis, E. Miller, and A. F. Miller, of Toronto.

Mr. R. G. Leigh described an observation of the dark transit of Jupiter's IV. satellite. At 10 o'clock on the night of the 27th February the satellite was well on the disc of Jupiter, and just below the S. belt. It appeared in his 6½in. Calver equatoreal as a conspicuous dark spot of a dark brown or chocolate colour, many shades darker than the belt, which was of a light reddish hue. When the shadow immerged the contrast in colour was very apparent, the satellite being still of the same tint, whilst the shadow was almost black.

Mr. R. C. Johnson, F.R.A.S., in observing the same transit through one of Cooke's 7-in. refractors, had been particularly struck with the dark colour of the satellite. It could, in fact, only be distinguished from its shadow by its smaller size and the

position it occupied on the face of Jupiter. Five large photographs were presented to the society by Mr. A. A. Common, and a monograph of the Orion nebula by Mr. W. S. Franks. A unanimous vote of thanks was accorded to the donors. Eleven gentlemen were elected members, and eleven candidates were proposed.

SELENOGRAPHICAL NOTES, APRIL, 1885.

By Thos. Gwyn Elger, F.R.A.S.

Condamine.—There are few lunar regions which contain more intricate and remarkable details than that which lies between the Mare Frigoris and the extreme northern border of the beautiful Sinus Iridum. Included within a comparatively small area we find all the accompanying features of a magnificent mountain system which in many respects is second in interest to no other on the moon's visible surface. What especially strikes the observer when scrutinizing these highlands soon after the sun has risen upon them is the marvellous complication of their subordinate features in the shape of deep branching valleys, depressions, and steep elevations which bear more than a superficial resemblance to like details associated with great terrestrial chains. This similarity is far more closely exemplified here than even in the south-western slopes of the Apennines, and were a map of a portion of this area constructed in relief, it might assuredly do duty for a model of the spurs, ravines, and multitudinous cross-valleys pertaining to many of the loftier elevations on our globe.

The fine ring-plain Condamine is situated near the extreme northern limit of this mountainous district, being less enveloped by rugged heights than its neighbour Maupertuis, on the south of it, though on this side its surroundings are very rough and hilly and are traversed by a network of curious valleys and crossed by five long well-marked spurs radiating from its southern and south-western border which gradually die out in the region north of Maupertuis. Beer and Mädler's representation of the country between Condamine and the Laplace promontory (Riccioli's Terra Pruinæ), though satisfactory as showing the broader features and giving a good idea of its general character, is weak and erroneous as regards Condamine itself, as is also Schmidt's drawing in several important particulars. Shroeter's solitary sketch, *Sel. Top. Frag. T. XXIV.*, is barely more than a diagram, while Neison (*map VII.*), fails to convey a just notion of the formation by omitting many important details which are

tolerably conspicuous, though to some of these he specially refers in the text. On the 24th February, between 7h. 30m. and 8h. 30m., the east longitude of the morning terminator being about 33°, I examined Condamine and the neighbourhood with a power of 350. The shadow of the extremities of the western wall extended quite across the floor, while near the centre that of the intermediate portion reached barely half way, its minimum breadth occurring at a point marked by a low hill or craterlet. Near the north end of the west wall two conspicuous notches, full of shadow, were remarked, and a little beyond these, towards the north, two depressions were seen on the very gentle outer slope of the border. In the interior a very distinct ridge was observed running parallel to the eastern wall, and at no great distance from it. On the following night, 25th February, at 8h., the floor was practically fully illuminated. Mädler's craterlet on the north side, previously enveloped in shadow, was an easy object, as was also the nearly central spot just referred to, which seems to be a craterlet. The eastern interior ridge was likewise well seen, and a second ridge was remarked, commencing under the western wall and curving round to the south wall. The two notches of the previous evening appeared as brilliant craterlets on the summit of the western border. North-west of Condamine lies a bright region which would afford ample employment for observers who wish to study the arrangement of craterlets. There are many linear groups of these objects hereabouts of various dimensions, and a delicate cleft in apparent connection with them, of which no indications are shown in any map. The spurs or ridges radiating from the south side of Condamine and their connection with the somewhat exceptional structure of this formation, are also features deserving special study.

In the Mare Frigoris, at about one-third of the distance between Condamine and Fontinelle, there is a small craterlet (? crater-cone) standing on an ill-defined milky-white patch, and there is another very similar object a little south of the latter formation. As the actual character of features of this type is not known, it would be well if observers, when they meet with them, would always carefully note their position and appearance, with a view to the possible detection of subsequent changes.

Central Range of Hills in Hercules.—Last November attention was drawn to observations made by Herr Haeckel, of Stuttgart, between February 27th and June 24th, 1882, and published in the *Selenographical Journal*, Vol. V., p. 92, relating to a horse-shoe-shaped range of hills on the floor of Hercules, immediately north of the brilliant central crater D. When these objects were first noted on February 27th they were two in number, "bright,

glittering peaks connected by a line, not always clearly seen, and then only with my highest power" (187). On March 1st, instead of the two spots previously seen, he noticed two bright crescent-shaped ranges of hills "not unlike the central range of hills in Atlas." On March 3rd, and again on May 27th, they were visible with a power of 80. The western portion of them was also seen on June 21st and 24th. Herr Haeckel regards all these objects as new from the fact that he now sees them readily with a power of 80, and because he detected no signs of them between September, 1881, and February, 1882, although Hercules was constantly under observation. However this may be, though for many years past I have observed and drawn the formation under various conditions of illumination, no details were ever recorded on the floor between the crater D and the north border, except two nebulous white spots, which appear to agree in position with the western members of Herr Haeckel's ring. At 8h. on 27th February, however, the eastern longitude of the morning terminator being about 69°, I easily perceived the horse-shoe ring with powers 200 and 350. It appears to consist of four or five hills, the most westerly of which stands some distance to the north-west of the crater D, and is the brightest of the series; the next, rather less obvious, is nearly due north of D, and lies about midway between its northern rim and the inner foot of the northern wall of Hercules. Then come two or three much fainter objects, which run up to the north-eastern border of D at a point where there is always a most brilliant spot (peak) under a high sun. These latter are the objects which have become more distinct of late, if any change has really occurred, for I find several records of the other two (westerley) spots made in 1872 with a power of 200 on a 4 in. achromatic. Though thus able to confirm the accuracy of Herr Haeckel's observations as to the existence of a ring of apparently isolated hills in the position he specifies which is very similar to the range of hills on the central part of the floor of Atlas, I cannot assert that it would have been seen as a whole unless I had very carefully searched for it. At any rate, these features are far too delicate on which to found even a suspicion of change. Their visibility is undoubtedly peculiarly liable to be affected by very slight changes due to illumination, so that unless they are caught up at certain times, no traces can be seen of them. They are objects which, being new to selenographers, should be diligently reobserved.

Kempston, Beds: March 19, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.
To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

SATURN.

Several observers have recently noticed Encke's division as very definitely visible under the form of a narrow black line curving round both ansæ. The obvious inference is, that this feature has become more conspicuous than during the previous opposition, when it was scarcely perceptible at all, and led to the expression of doubt, in certain quarters, as to its continued existence.

Before the planet declines much further in point of position it is, therefore, most desirable to examine this feature of the rings with scrupulous care. The degree of prominence with which it is visible certainly appears subject to variation, but there are not many nights when the edges of the rings are sufficiently free from rippling to allow a steady view of detail.

In my paper on Saturn (*Register*, November, 1884, p. 267) my remarks may lead to the inference that the results of the 26-inch Washington Refractor can be safely adopted as the corrective to certain features alleged to have been detected in smaller instruments. But this need not necessarily be so, for my firm belief now is, that, as a rule, large aperture is no advantage in such work, because of the greater interference from atmospheric vapours. This conclusion is irresistible from comparisons instituted between telescopes of many different sizes.

W. F. DENNING.

Bristol: March 24, 1885.

Sir,—Before this wonderful planet has taken his departure for the present season, I am induced to inquire whether any observer can confirm an impression that I have for some timed received, that the outermost ring is less contrasted in brightness with its neighbours than in former years. There cannot be a question as to its inferior luminosity at all times; but I have a strong suspicion that of late either A has been somewhat gaining, or, more probably, the outside zone of B losing in light. Memory, however, is so unsafe a guide in such matters that I am far from feeling confident, and only suggest the idea for confirmation or contradiction at the hands of others. The researches of M.

Trouvelot, recently published in the *Bulletin Astronomique*, seem to leave little doubt as to the fact that the details of the ring-system are subject to considerable change.

Yours obediently,

T. W. WEBB.

THE RED SPOT ON JUPITER.

This spot has rather increased in distinctness of outline and colour lately, especially at the following end. Following the spot, in the same latitude, there is an unusually definite mark, which commenced as a straight gray line, and is now increased in size, and becoming red. Its size is about half the length of the old red spot, by about a third of the width, and is on the meridian about an hour after the red spot. This should be closely watched, as it may develop into a similar marking.

N. E. GREEN.

39, Circus Road, St. John's Wood :
March 23, 1885.

OCCULTATION OF ALDEBARAN.

Sir,—As the recent occultation of α Tauri has attracted some attention, perhaps the following record may possess some interest for many of your readers. I may add that both the disappearance and reappearance were seen here, the former at 5h. 9m. $\pm$, and the latter at 5h. 55m. 13s. G.M.T.

I am, sir, yours truly,

J. GLEDHILL.

Bermerside, Halifax : March 7, 1885.

Joseph Gledhill, Esq., F.R.A.S.

Dear Sir,—Observing the occultation of Aldebaran, for the first time, with my $6\frac{1}{2}$-in. reflector, I was much surprised to see the star projected on the moon previous to *reappearance*. I was so unprepared for this, from the conversation we had previously, and the remarks by "F.R.A.S." in *Knowledge*, that it did not dawn upon me at first what the bright spot (a white point of light), about 3 star diameters from the bright edge of the moon, could be. While I was puzzling myself with the phenomenon, however, I saw the bright spot pass on to the edge of the moon to the point where the star reappeared, and thus was the enigma solved.

Yours faithfully,

THOS. H. FARRAR.

45, Savile Park, Halifax : Feb. 24, 1885.

DUN ECHT CIRCULAR, No. 93.

M. Raoul Gautier, of Geneva, communicates some most interesting particulars respecting the return of Tempel's periodic comet, 1867, II.

From M. Gautier's calculations it seems that the disturbing action of Jupiter, in the interval 1879 to 1885, will retard the comet's approach to the sun by 148 days, besides considerably enlarging the orbit while decreasing its eccentricity. As the comet will not approach the earth within one and a half times the distance of the sun, it is doubtful if it will become visible even in the most powerful telescopes. Its calculated place, however, is shown in the following ephemeris, in which the unit of brightness (I) is that of the comet when last observed by M. Tempel, at Florence, on July 8, 1879.

Elements for 1885·0, osculating Sept. 19, 1885.

$$
\begin{aligned}
M_0 \text{ Sept. } 19{\cdot}0 &= -1^\circ\ 1'\ 28''{\cdot}94 \\
T &= 1885, \text{ Sept. } 25{\cdot}7649 \text{ Berlin M. T.} \\
\mu &= 545''{\cdot}3073 \\
\phi &= 23^\circ\ 53'\ 57''{\cdot}0 \\
i &= 10\ \ 50\ \ 27{\cdot}2 \\
\Omega &= 72\ \ 28\ \ \ 7{\cdot}7 \\
\omega &= 168\ \ 57\ \ 53{\cdot}3 \\
\log a &= 0{\cdot}542244
\end{aligned}
\quad \left.\begin{matrix} \\ \\ \\ \\ \\ \\ \\ \end{matrix}\right\} \text{ Mean Equinox 1885·0 (for } i, \Omega, \omega)
$$

Ephemeris for Berlin, Midnight.

1885		R. A. 1885·0 h.	m.	s.	Decl. 1885·0 °	′	log. Δ	log. r	I
Mar.	13·5	12	11	41	+17	44·4	0·1899	0·4005	0·198
	17·5	12	8	30	18	5·3	0·1855	0·3979	0·205
	21·5	12	5	12	18	23·4	0·1822	0·3954	0·210
	25·5	12	1	51	18	38·1	0·1801	0·3928	0·215
	29·5	11	58	31	18	49·2	0·1791	0·3902	0·218
Apl.	2·5	11	55	17	18	56·3	0·1792	0·3877	0·221
	6·5	11	52	14	18	59·1	0·1803	0·3851	0·222
	10·5	11	49	24	18	57·7	0·1824	0·3826	0·223
	14·5	11	46	52	18	51·8	0·1855	0·3801	0·222
	18·5	11	44	40	18	41·6	0·1894	0·3776	0·221
	22·5	11	42	52	18	27·1	0·1940	9·3751	0·219
	26·5	11	41	29	18	8·7	0·1993	0·3726	0·216
	30·5	11	40	32	+17	46·4	0·2051	0·3702	0·213

Lord Crawford's Observatory,
Dun Echt: 1885, March 13.

REVIEWS.

A Catalogue of known Variable Stars, with Notes and Observations. By J. E. Gore, M.R.I.A., F.R.A.S., &c. A paper read before the Royal Irish Academy, January 28, 1884, and reprinted from the "Proceedings," Second Series, Vol. IV., No. 2 (Science), (Fifty copies only, reprinted by the Academy for the Author). Dublin: University Press, 1884.

A very useful pamphlet, the fullest, we imagine, in the English language on the subject, to which, we believe, the author has long given his attention. The number of variables is 191, which he thinks will be found to include all those stars which have been certainly proved to be variable in light. The periods, however, and the light curves of some have not as yet been accurately determined, and require continuous observation

by observers in different parts of the world. These, which are indicated, are twenty-eight in number. The Catalogue is followed by notes of Professor Schönfeld and other authorities and the author himself on some of the more remarkable variables. The positions have been brought up to 1880.0, and the epochs of maxima and minima (one or both), chiefly from Schönfeld's Catalogue, are given to over 100 stars. Particulars of several of the new variables discovered at Cordoba have been taken from Dr. Gould's *Uranometria Argentina.* "Observing," p. 166, l. 1, should be "obscuring" (an erratum).

In the *Sidereal Messenger* for August, 1884, Carleton College Observatory, is a paper by the editor, Mr. Wm. W. Payne, on the Planets and Cyclones. Observing that our knowledge of the tremendous forces of the sun is too imperfect for their laws to be stated for the meteorologist or the weather-observer, he says that recent interesting coincidences invite astronomers to this study. The year 1883, very probably, marks the maximum of a passing sun-spot period. In some locations certainly last year will long be remembered for its cyclones and tornadoes. In a group of nine Western States, the Signal Service reports the occurrence of over forty of these dreadful storms within the space of four months. . . . The indications are that the storms of 1883 were more in number and more destructive than in any years immediately before." For sound deductions, however, we need to have the meteorology of the entire globe before us.

In the same number is an account of the observation by Mr. Wilson, acting astronomer at the Cincinnati Observatory, of Venus in conjunction, July 11th. By shutting out the light of the sun from the object glass and reducing the aperture of the eye piece to about one-thirtieth of an inch he obtained a very distinct view of the planet. The crescent on the north limb was very brilliant and much thicker than he had anticipated. The cusps extended considerably more than half-way around the planet. At times they seemed to flash out, so as to cover more than two-thirds of the circle. The janitor, who has been accustomed to bright light from counting sun spots for six years, was positive that he could see the complete circle. Mr. Wilson thought he caught a glimpse of a thread of light opposite the crescent once or twice, but was not satisfied of its reality.

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
April. 1	+66 39 E.	April 11	−55 16 E.	April 21	+ 2 44 M.
2	54 28	12	67 28	22	− 9 29
3	42 16	13	79 41	23	21 41
4	30 4	14	+88 8 M.	24	33 53
5	17 53	15	75 56	25	46 5
6	5 41	16	63 44	26	58 18
7	− 6 30	17	51 33	27	70 30
8	18 42	18	39 19	28	82 43
9	30 53	19	27 8	29	+85 6 E.
10	43 5	20	14 55	30	72 53

Moon farthest from the Earth, April, 6, 4h.
,, nearest to ,, ,, ,, 18, 3h.

ASTRONOMICAL OCCURRENCES FOR APRIL, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Wed	1		Sidereal Time at Mean Noon 0h. 40m. 2·20s.	2nd Oc. D. 2nd Ec. R. 4th Tr. I.	7 11 11 53 11 14 59	9 15·0
Thur	2		Sun's Meridian Passage 3m. 31·57s. after Mean Noon			9 10·8
Fri	3			1st Tr. I. 1st Sh. I.	15 5 16 2	9 6·6
Sat	4			1st Ec. D. 1st Ec. R.	12 13 15 26 56	9 2·5
Sun	5			1st Tr. I. 1st Sh. I. 3rd Sh. E. 1st Tr. E.	9 32 10 31 11 52 12 50	8 58·4
Mon	6			1st Oc. D. 1st Ec. R. 1st Tr. I.	6 40 9 55 35 14 28	8 54·2
Tues	7	2 42	☾ Moon's Last Quarter	3rd Oc. R. 3rd Ec. D. 3rd Ec. R.	10 45 11 13 55 14 40 51	8 50·1
Wed	8	15 48	Near approach of 14 Capricorni (5)	2nd Oc. D. 2nd Ec. R.	9 34 14 29 22	8 46·0
Thur	9					8 41·9
Fri	10			2nd Sh. E. 4th Ec. D.	8 40 14 37 10	8 37·4
Sat	11			1st Oc. D.	14 1	8 33·8
Sun	12			1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	11 22 12 25 13 41 14 45	8 29·8
Mon	13	18	Conjunction of Moon and Mars 0° 12′ S.	1st Oc. D. 1st Ec. R.	8 29 11 50 22	8 25·7
Tues	14	17 51 8	● New Moon Conjunction of Moon and Venus 0° 6′ N.	1st Tr. E. 1st Sh. E. 3rd Oc. D. 3rd Oc. R.	8 9 9 13 10 43 14 22	8 21·7
Wed	15	19	Conjunction of Moon and Mercury 6° 21′ N. Illuminated portion of disc of Venus = 0·996 Illuminated portion of disc of Mars = 0·993	2nd Oc. D.	12 0	8 17·7
Thur	16		Sidereal Time at Mean Noon 1h. 39m. 10·49s.			8 13·7

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Fri	17		Sun's Meridian Passage 0m. 32·28s. before Mean Noon	2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	8 21 9 2 11 15	Jupiter. 8 9·7
Sat	18	13	Conjunction of Moon and Saturn 4° 1′ N. Saturn's Ring: Major axis=39″·01 Minor axis=17″·76	3rd Sh. E. 4th Tr. E.	8 43 11 36	8 5·7
Sun	19			1st Tr. I. 1st Sh. I.	13 12 14 20	8 1·7
Mon	20	10 59 11 20	Occultation of λ Geminorum (3½) Reappearance of ditto	1st Oc. D. 1st Ec. R.	10 19 13 45 15	Arcturus. — 12 13·5
Tues	21	11 20	☽ Moon's First Quarter	1st Sh. I. 1st Tr. E. 1st Sh. E.	8 49 9 59 11 8	12 9·5
Wed	22	9 4 10 4	Occultation of B.A.C. 3122 (6½) Reappearance of ditto	1st Ec. R. 2nd Oc. D.	8 14 14 28	12 5·6
Thur	23	6 46 7 42 7	Occultation of π Leonis (5) Reappearance of ditto Conjunction of Moon and Jupiter 4° 37′ N.			12 1·7
Fri	24	11 7 11 58	Occultation of *d* Leonis (5) Reappearance of ditto	2nd Tr. I. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	8 34 10 57 11 30 13 52	11 57·8
Sat	25			3rd Sh. I. 3rd Sh. E.	9 6 12 41	11 53·8
Sun	26	8 28 9 39	Occultation of B.A.C. 4255 (6½) Reappearance of ditto	2nd. Ec. R.	8 59 32	11 49·9
Mon	27	15	Inferior conjunction of Mercury and Sun	4th Ec. R. 1st Oc. D.	8 55 43 12 11	11 45·9
Tues	28	18 14 7	○ Full Moon Conjunction of Venus and Mercury 1° 41′ N.	1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	9 31 10 44 11 51 13 3	11 42·0
Wed	29	15 46 16 55	Occultation of o¹ Libræ (6) Reappearance of ditto	1st Ec. R.	10 8 58	11 38·1
Thur	30			1st Sh. E.	7 32	11 34·1
MAY Fri	1	15 14 16 22	Occultation of 29 Ophiuchi (6) Reappearance of ditto	2nd Tr. I. 1st Sh. I.	11 5 13 33	11 30·2

THE PLANETS FOR APRIL.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	1 46 14	N.12 48	6″·4	1 6·0
	9th	2 22 33	N.17 11	8″·0	1 10·8
	17th	2 35 42	N.18 17½	10″·0	0 52·4
	25th	2 26 54	N.15 11	11″·6	0 12·2
Venus ...	1st	0 18 50	N. 0 28½	9″·8	23 34·9
	9th	0 55 12	N. 4 26½	9″·8	23 39·7
	17th	1 31 53	N. 8 19	9″·6	23 44·9
	25th	2 9 11	N.11 59½	9″·6	23 50.6
Jupiter ...	1st	9 56 29	N.13 52½	39″·6	9 15·0
	9th	9 54 56	N.13 59½	38″·8	8 41·9
	17th	9 54 9	N.14 2½	38″·0	8 9·7
	25th	9 54 7	N.14 1½	37″·2	7 38·2
Saturn ...	1st	5 12 37	N.21 53	16″·0	4 31·8
	9th	5 15 23	N.21 58	15″·8	4 3·1
Uranus ...	3rd	12 2 24	N. 0 35	4″·0	11 12·6
	19th	12 0 7	N. 0 50	4″·0	10 7·4

Mercury sets nearly two hours after the sun at the beginning of the month, the interval decreasing.

Venus is too near the sun to be favourably observed.

Jupiter sets about an hour before the sun rises, on the 1st, the interval increasing.

Saturn sets three quarters of an hour after midnight at the beginning of the month, and afterwards earlier each night.

Books received:—The Sun and his Phenomena. By the Rev. T. W. Webb. Longmans.—Astronomische Nachrichten.—Sidereal Messenger.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1884.	To Dec., 1885.
Dallmeyer, J. H.	Freeman, G. T.
Dobie, Dr.	Lettsom, W. G.
Glaisher, J. W.	Spitta, E.
Metford, W. E.	
Webb, Rev. T. W.	

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 269. MAY. 1885.

ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

The April meeting was held on Friday, the 10th, at Burlington House.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut.-Col. Tupman.

The following gentlemen were balloted for and duly elected Fellows of the Society:

Lieut. S. G. Burrard, R.E., Dehra Dun, India.
The Rev. J. H. Honeyburne, M.A., 97, Mulgrave Street, Liverpool.
Jas. McKerrow, Esq., Surveyor General of New Zealand, Wellington, N.Z.
John A. Westwood Oliver, Esq., Braehead House, Lochwinnoch, N.B., and 13, Bruton Street, Berkeley Square, W.
Major Harry T. Watson, Langley House, Slough, Bucks.

Col. Tupman read the minutes of the previous meeting, which were duly confirmed.

Captain Noble said, shall I be out of order in asking why the *Monthly Notices* have not reached the Fellows this morning?

Mr. Knobel: The *Monthly Notices* were sent off by yesterday evening's post and they ought to have been in the hands of all the country Fellows this morning. They were in my hands this morning and they ought to have been in the hands of other Fellows.

Several Fellows said they had received their copies of the

Notices that morning, and others that they had received them the previous evening.

Captain Noble: It is running it rather fine, if they are only posted on the day before the meeting.

Colonel Tupman said that the numbers had been rather heavy of late.

Mr. Knobel: I am very glad indeed that Captain Noble has brought this matter before the Society, because it is one we ought to have a clear understanding about. In the preparation of the *Monthly Notices*, there is a good deal of work for which the Editors are entirely responsible. In the number just published, there is a long diagram which has taken some time to correct and prepare. There are also long tables which have likewise taken time to correct, and pass through the press, and it is a question entirely for the Fellows to decide whether the *Monthly Notices* shall be so hurried through the press that without doubt the numbers shall be in the hands of Fellows the day before the ordinary meeting, or whether we shall endeavour to exercise so much care in passing the *Monthly Notices* through the press as shall ensure the accuracy of their contents. There has been a month since the last meeting, and Easter has occurred during that month and therefore a delay has taken place in printing establishments. Then again the number is a thick one, as you are all aware, and it has taken a considerable time to read through. I must call the Fellows' attention to the point that not only have the proofs to be read through, but when they are completed, revised, and passed, the *Monthly Notices* have to be read through in page by the Secretaries. It is certainly done so far as I am concerned, and I believe also with all other secretaries, the reading through of the *Monthly Notices* is done at once, without any delay, and returned by the following day's post, it can hardly be done in less than a day without passing over some possible errors. It is in the hands of the Fellows whether the *Monthly Notices* shall be hurried through the press, so that errors may possibly creep in, or whether we shall continue to get them out as early as possible in the best manner.

Mr. G. F. Chambers said that the explanations which Mr. Knobel had so clearly given had been the periodical explanations made during the last 20 years at tolerably regular intervals, and he could not conceive that they were at all satisfactory to the great bulk of Fellows. Great care should be taken, and he thought great care was taken, in the preparation of the *Monthly Notices*, but he believed it would be a lesser evil to leave out some of the longer and least important papers, and get the *Notices* out earlier, than delay their publication.

Captain Noble said that when Mr. Proctor edited the *Notices* they were out something like a week before the meeting.

The President said he felt great interest in the *Monthly Notices* and their punctual appearance, and he regretted very much that they did not appear as regularly as some of the Fellows seemed to think they ought to do. He had been editor of the *Monthly Notices* for a short time, and it would hardly be believed the difficulty which he experienced in getting the proofs returned from the different authors, so as to be able to get them in proper order for publication. Not only that, but there are times in the year, as at the present, when such a festival as Easter occurs, when there is a holiday week, and then some delay must necessarily take place. When he received his copy of the *Monthly Notices* on the previous evening he thought it was a marvellous thing that it should appear so soon, considering that it contained 80 pages, and was full of very important papers. Those accustomed to reading proof-sheets containing tabular matter would know that they could not be got through in a short time. They required careful reading, unless they were to be published full of errors. As, however, Mr. Knobel had said, it rested entirely in the hands of the Fellows. If Fellows were desirous that the *Monthly Notices* should appear punctually, then of course they must be got out, and the Secretaries would do all they could to make them appear before the meeting, but then it might be at the expense of accuracy.

Colonel Tupman said he should like very much to hear from Captain Noble what difference it made to him whether the *Monthly Notices* arrived before the meeting or not.

Captain Noble said it made this difference, that very often subjects arose at one meeting having reference to papers read at the preceding meeting, and any one who was not present at that meeting would not know anything about the matter under discussion.

Professor Adams hoped the Council or the editors would never be so ill-judged as to send out the *Monthly Notices* without taking the very greatest care to ensure accuracy. The speedy appearance in print was a matter of much less importance.

Mr. Ranyard said he was aware there was difficulty about the printing of the *Monthly Notices*, especially with their present printers. He thought there were two ways in which hurry might be avoided. Either papers which were long, or ought to be illustrated, might be postponed at the discretion of the secretaries, or the *Monthly Notices* themselves might be postponed until after the meeting. It seemed to him that it would be a lesser evil if the longer papers were postponed, and if those which were easily

got through were printed in time for the meeting, and in the hands of the Fellows two or three days before.

The subject then dropped.

Colonel Tupman said that 63 presents had been received since the last meeting, amongst those calling for special notice was a beautiful series of photographs of the Orion Nebula, taken by Mr. Common, who, he hoped, at a later period in the evening, would explain them himself.

The thanks of the Society were returned to the respective donors.

Mr. Green was called upon to exhibit a drawing of Saturn. He stated that he thought it would be interesting at this time in regard to the question of the supposed changes upon the planet's surface. Certain appearances had been reported here, and from America, and from France, which indicated that a change had taken place recently upon the surface of the planet. The period of opposition had now passed and there would be no equally good opportunity for some years of observing the planet with the ring in the same position. The drawing he showed had been made with an 18-inch speculum, with powers varying from 250 to 400. He had taken advantage of all the evenings that had been favourable, but could not say that very good drawings had resulted; and it had been difficult to observe details on Saturn. In past years he had considered that Saturn was a very favourable object to look at with a telescope, in consequence of the general sharpness of the detail, it always bore a higher power than was possible in observing Jupiter; but during the last opposition he was not able to get such good definition, and began to suspect his own sight or the instrument; but on turning the instrument upon Jupiter, lying lower towards the horizon, he was able to see all he desired, and the details came out beautifully. The opposition for 1883 was a finer one for drawing purposes, Saturn then bore a power of 400, all the details could be seen charmingly, and some of the markings appeared more prominently than during the last opposition. Mr. Green had not been able to observe Encke's division in the outer ring, although he had some favourable opportunities for observation during the recent, as well as during the preceding, opposition. He was disposed to think that the appearance which had been described was due to inequalities of tint in different parts of the outer ring, which when seen with a smaller aperture might give the appearance of a division. Many drawings also showed a sharp line of demarcation between the inner ring and the crape ring; but to him it had always appeared that the crape ring merged gradually into the brighter ring outside it. He had at

no time seen a bright spot at the southern pole of the planet, such as was shown in some drawings; and there was but little detail visible from the polar cap to within a short distance of the first dark belt. The markings on the ball of the planet, which were shown in his drawings, had been verified by Mr. Edward Nelson, who observed with the 18-inch reflector, and Mr. Arthur Cottam had confirmed the appearances of the outer ring by observations at Watford.

Mr. Ranyard said he concurred with Mr. Green as to the gradual merging of the crape ring into the brighter ring outside it. The dark belt, as shown in Mr. Green's drawing, was somewhat broader, and lower down towards the equatorial regions than it appeared to him, and he thought that the polar cloud cap was more uniform in tint than it appeared in Mr. Green's drawing. The edge of the dark belt appeared to him not to be uniform, but he could not say that he saw the crenilations drawn by Mr. Green.

Mr. Knobel said there had evidently been a great change in some of the appearances since last February twelve months.

Captain Noble said it might be interesting to possessors of moderate optical means to know that he had seen the northern dark belt quite distinctly with a $4\frac{2}{10}$-inch aperture, as against Mr. Green's 18-inches. Many other things which Mr. Green saw he could also see with his smaller telescope.

The President: If no other gentleman wishes to make any remark we will return thanks to Mr. Green. I am sure you will do so, as he has gone into his subject, in a remarkable and most interesting manner, and he has given us, and myself particularly, a great deal of instruction as regards the planet at the present time.

We are privileged this evening, in having amongst us one of our distinguished associates, Professor Langley. We all know Professor Langley, as the Director of one of the most important observatories in America, the Allegheny Observatory. He has done most important work there, and I think he will allow us to ask him to tell us to-night something of the work he has been engaged upon.

Professor Langley: Mr. President, I did not anticipate this honour you have done me to-night in asking me to speak, and I am quite unprepared to make any formal presentation of the woık of the observatory you have so kindly alluded to. I can only say, that for the last five years, we have been increasingly engaged in studies connected with the solar energy, regarded, not merely as light, but as heat, and in connection with that we have been investigating the obstacles which our atmosphere offers to the

transmission of those radiations. It is some five years ago we began to study more particularly those regions of the solar infra red spectrum, which lie beyond the portions which Captain Abney has so successfully photographed, and we had carried our observations to a point which, I think, had hardly been reached by others, when we found the difficulties of the atmosphere prevented us from going much further with our then means. Those means consisted largely of such instruments as the thermopile, the most delicate we then possessed, and we were occupied for a considerable time in perfecting an apparatus more sensitive and perhaps more accurate, with which to carry those investigations further. After many years' study with the pile, we seemed to have reached the limits of its capacity. Now we have an instrument which I have called the bolometer, and which consists of an extremely fine wire, in which an electric current passes, and on which, the heat falling, causes a change in the conductivity, and a consequent deflection in the galvanometer. We commenced mapping the spectrum in the infra red, using for that purpose a grating which should give us a spectrum on the wave length scale, the prism—as is well known—giving us a spectrum which differs with every instrument employed. We know of the excellent work which the prism does, and is doing, but for the point immediately in hand the grating seems to be preferable. The heat, however, in the grating spectrum is in certain portions about $\frac{1}{400}$th of what it is in like portions of the prismatic one, and an instrument of corresponding delicacy was required to measure in the infra red, so that the possession of the bolometer has opened a quite new field to us. Another object we had in view was to find what obstacle our atmosphere opposed to the radiation of the solar and lunar energy, and that of other celestial bodies, and that could best be done, as it seemed to us, by studying the spectrum in portions so narrow that they might be treated as individual rays, which this instrument—consisting of a platinum wire, in some cases less than one-thousandth of an inch in diameter, in others, of a platinum tape less than $\frac{1}{25000}$ of an inch—was well able to do. Without detaining you with a longer account of the preliminary work on the apparatus (for it was extremely tedious in its preparation) I may say this, it gave us finally an instrument which could measure amounts of heat in such excessively narrow spaces as the separate Fraumhofer lines, and make such explorations in the spectrum as gave us promise of ascertaining more about the effect of the atmosphere than had been done before. In the course of these investigations there was fitted out from the Observatory, at the cost of a citizen of Pittsburgh, and with the

further aid of the United States Government, an expedition to ascend one of the highest mountains in its territory in the Sierra Nevadas; and to that spot we carried this apparatus, and other very elaborate adjuncts, such as a large siderostat by Mr. Hilger, which had been most kindly examined for us by Mr. Christie, the Astronomer-Royal. We pursued our work upon the mountain, at altitudes of from 12,000 to 15,000 feet, and there discovered, I think, spectral regions before undiscriminated, and which occupied in the wave length scale an extent greater than all previously known, and we continued our work after our descent, mapping to a wave length of nearly 30,000 in Angstrom's scale.

These results are already before the public, but we have more lately carried on researches on the radiation from the moon, and have found it necessary, in elucidation, to measure the wave lengths given out by terrestrial bodies at very low temperatures, and have incidentally carried down, on artificial sources, our researches to wave-lengths of from (*probably*) 80,000 to 100,000. But as we have found no such rays come from the sun, we seem to be driven either to the conclusion that the earth's atmosphere is impervious to all solar radiations below 30,000, or else the alternative conclusion, which at first sight is a surprising one, and one we are not disposed to readily accept,—that the solar radiation does not contain any such wave-lengths within it.

We have gone on with observations of the kind which I am detailing, and which I fear lie too far from the field of the classic astronomy to interest most gentlemen present, but which to the student of what has been somewhat ironically called "inexact astronomy," possess a general interest. We have more recently been engaged in measuring anew the energy of the sun, obtaining a materially larger value of the solar constant than that now entertained. In studying on homogeneous rays, the absorption of the earth's atmosphere, we have found this absorption to be much larger than hitherto supposed, nearly all observations by old methods having made it, roughly speaking, 20 per cent.; but these new methods, if justified by the result, lead us to think it is nearly double that, a difference which seems at first sight irreconcilable with the known exactitude of experimenters who have already worked in that field, but which really involves no impugnment of the accuracy of their observation, but only of their deductions. We are still pursuing these researches, which all deal with the energy which comes to us from heavenly bodies in the form of heat, and we are just at present studying the character of the radiation from the moon; and I may mention, as an illustration of the delicacy of the apparatus we are now using, that while former most skilful observers have found it difficult to detect any

lunar heat at all, we find ourselves able to get an amount of heat with a small mirror of 8 inches in diameter that will drive the needle many times through the length of the scale of the galvanometer, so that we are obliged to damp it in order to control it, and further, we find ourselves able to take that heat through a train of rock salt prisms, and lenses; draw it out into a heat spectrum. and measure in different parts of it, both visible and invisible. Such investigations, and others intended to utilize the effects of this solar energy, are still going on. All these last have to me a special interest, and I hope that they may lead us to something of general use.

I may remark that in ascending to the height of from 12,000 to 15,000 feet, I have been lately struck with the immense advantages that those altitudes afford, not only to the observer in Physical Astronomy but in other departments, especially in the definition for double star observation, the admirable clearness and transparency in the atmosphere for the purpose of photographing the heavenly bodies, and the wonderful appearance of the nebulæ at such heights, and also the gain in exactness of meridian work, and in observations of precision generally. I have succeeded, through the representation of the War Department, in obtaining a government reservation of an elevated tract of 100 square miles in the Sierra Nevada Mountains for the purposes of Astronomical Science, though I fear that there is no immediate prospect of an observatory being built. We have that, however, in the future still to come, and I think that with the now successful inauguration of the Lick Observatory at the lower elevation of between 4,000 and 5,000 feet there is a prospect (I hope not too far distant) of a possible erection of a physical observatory on Mount Whitney.

We are approaching, I trust, to those conditions foreshadowed so long ago by Sir Isaac Newton, who saw that we must finally, in order to pursue our observations to advantage, get above this turbid atmosphere in which we are now working, under difficulties which we may thus triumph over; difficulties every observer is now aware of in every field of astronomical work.

Mr. Stone: Some years ago I experimented a little in this matter, and found that the difficulty I experienced, was not the want of sensitiveness in my instrument, but rather in one respect an over sensitiveness. I found in the determinations I was making, it was perfectly hopeless to do anything in the way of quantitative results on account of the atmosphere. So long as I made use of one delicate thermopile for observations, I found I was simply registering every variation of the absorption of the earth's atmosphere between the instrument and the sky; and I

found it necessary for this experiment to get rid to some extent, or to a great extent of the changes of the atmosphere, by using two opposed thermopiles, one on the star and the other on the atmosphere near by, and then noting the difference of the readings, I found I could get quantitative results with some degree of accuracy and very satisfactorily. I should be glad to know whether Professor Langley in his work with an instrument, probably of much greater sensitiveness than that I was using, has not experienced this same kind of difficulty. Undoubtedly, I should find I got a certain amount of heat from a star, when the atmosphere was perfectly clear, but it was impossible for me to attempt to obtain anything in the nature of quantitative results, until I had got rid of the change which is continually taking place with regard to atmospheric absorption ; and it occurred to me that any instrument more sensitive than I used must have aggravated that evil to a very considerable extent indeed.

Mr. Common : I should like to ask Mr. Langley to give us, if possible, a little more information as to the construction of the bolometer. Is it platinum deposited on glass, or is it wire of platinum spread out, or is it metal spread on glass, or what is it? If it is not too much to ask, such information would, I think, be appreciated.

Professor Langley : In answer to Mr. Common, I would say, as to the construction of the bolometer, the first we constructed were from iron, which had been made in a contest of skill between the American and English iron masters, in which 15,000 sheets, laid one on another, made up one English inch. From that thin iron we cut our first strips ; but afterwards we found ourselves driven to the use of platinum. Iron was good while it lasted, but it got rusty. The bolometer is now made with strips of platinum, which are from $\frac{1}{25000}$ths to $\frac{1}{50000}$ths of an inch thick, but ordinarily $\frac{1}{25000}$th. The width of the strip is from $\frac{1}{120}$th to $\frac{1}{250}$th of an English inch, and it is about $\frac{1}{4}$-inch long, so it is like an extremely minute piece of tape, and when one looks at it, it is almost comparable to the wire of the ordinary reticule, though it is far thinner. When it is used for measuring the spectrum, this wire, is passed down it parallel to the lines, and as it touches each spectral line, where the eye sees blackness, the wire feels the cold, and there is a corresponding resistance of course, and equally so whether the eye sees or not. We have it now made in the form of a reticule, with a single wire, and controlled by a micrometer screw. We are using it also in observations of the moon, and here it is stretched backwards and forwards several times, so as to occupy a certain amount of area. In answer to another

question, which was raised by Mr. Stone, I will say we have experienced the difficulty which he has alluded to; that is, we found that variations of about 8 to 10 per cent. in an entirely (optically) clear sky constantly presented themselves. I mean to say, that clouds passed which the eye did not recognise, but the instrument did, still there were a portion of the variations which have been referred to we found we could eliminate, by putting the instrument in a hollow tube with diaphragms, as at the bottom of a sort of well. By numerous diaphragms, arranged as I am now drawing them on the board, we entirely shut out the air currents, which are the cause of a great deal of the trouble. The atmospheric variations presented and do present, obstacles, but that has been overcome by the patience of many observers, and by taking the average work from year's end to year's end.

Mr. Stone: Might you not adopt the same expedient I did, and have two reverse bolometers?

Mr. Langley: We have been to a great deal of labour in that. There are certain technical difficulties peculiar to this form of instrument, but we have finally been led to try it in the form I have indicated.

The President: We have all been delighted to hear the remarks of Professor Langley. I can assure him that his researches are not strange to us, because we have for many years had our eyes directed towards the Allegheny Observatory, and he has sent many papers to this country in explanation of the various researches on which he has been employed. I am happy to say he has kindly sent me some of those papers, and that I have read them with the greatest profit and pleasure. After hearing his remarks to-day, I am confident that all present will leave this room feeling they have learnt something from them. I will ask you to return thanks to Professor Langley by acclamation. (Applause.)

Mr. Common exhibited some enlargements of photographs of the nebula of Orion, taken in 1883. He said that they had been enlarged for him by Mr. Lockyer, who had given him these copies to present to the Society. The enlargements had been made from photographs of the nebula with different exposures, with the view of making out the differing contour lines of equal brightness in the structure of the nebula. The time of exposure varied from a few minutes to 60 minutes. Each duration gave a negative which showed a varying amount of detail, the longer exposures bringing out fresh details in some parts, but obscuring others. What an exposure of 160 minutes would produce he did not know, but he believed that while some portions would be lost, others would come out that were at present invisible. The power of photography was of such a character that it would show

on paper or glass what the eye would never be able with any instrument to appreciate; and he had no doubt that if they could use the 60 minutes' photograph in the same way as they could the one minute photographs, and if they could get in 60 *minutes* as much in proportion as they got in one minute, they would not be able to recognize what they were looking at. Stars which had never yet been seen would be brought out by photography, and he was anxious to see some ardent student of photography step to the front and take the task in hand.

Mr. Knobel read a paper from Lord Crawford, *On an Observation of Jupiter*, on Feb. 18, 1885, made at Dun Echt by Dr. Copeland, when the shadow of Satellite No. 1 was seen on the planet. The transit of the second satellite the next day might be attended by a partial concealment of the shadow.

Mr. Knobel remarked that there was no novelty in the observation recorded. In 1874 he had sketched the satellite as concealing half of its shadow in a way very similar to one of the sketches accompanying Lord Crawford's paper.

Captain Noble explained a paper *On the occultations of B.A.C.* 3529, 43 *Leonis, and B.A.C.* 3836, his object being to bring to notice what to him was an unfamiliar phenomenon. The first and last, and especially the last star faded out in a way which suggested the idea of an objective disc instead of a diffraction disc. They had heard of double stars too close to be separated by any optical power, being discovered by their behaviour during occultation at the moon's limb, and he brought forward this observation in order that the stars might be observed by large telescopes, to see whether they were double.

Mr. Chambers: May I ask what is the result of the poll which has been taken?

The President: You will find the result posted up, but I will read it. The votes in favour of holding the meeting at 8 o'clock are 104; in favour of 5 o'clock, 125; and 149 Fellows have declared themselves neutral. For other hours there are 16 votes, and 100 Fellows have not replied.

Mr. Chambers: What is to be the practical result?

The President: Nothing is decided at present.

The following paper from Col. Tupman was also presented:—*Occultations of stars by the moon in the years* 1876-80, *and resulting final equations between the errors of the tables and the errors of observation.*

The meeting adjourned at 10 o'clock.

THE LIVERPOOL ASTRONOMICAL SOCIETY.

The seventh meeting of the Session was held on the 13th April; the Rev. T. E. Espin, B.A., (president) in the chair. The Rev. J. H. Honeyburne, M.A., F.R.A.S., read a paper "*On evenings in town with a small telescope.*" He said it was generally supposed that astronomical observation required an exceptionally good position, besides other conveniences; but he had found a 3¼-inch telescope, in a little back yard in Liverpool, quite sufficient to render the science a most delightful pursuit. He had noticed that members generally went in for a study and comparison of star magnitudes, or for an elaboration of mysterious hieroglyphics, supposed to represent the bottom of some ring plain on the moon. No doubt this was all very important and interesting to the initiated; but he made no claim to originality, and would merely give an outline of the colours of some stars, and the closeness or faintness of others. The tints of many of the stars were most lovely. β Cygni, and δ Cephei, he gave as examples; whilst the orange and blue of γ Persei fully entitled it to rank amongst the flowers of the sky.

Mr. T. G. Elger, F.R.A.S., hoped the important communication from Dr. Klein, read at the last meeting, would direct the attention of selenographers to the curious dark markings south of Copernicus. Since the publication of Schmidt's work he had carefully scrutinized this region, though with not much result, except that on the 3rd November last he had glimpsed two very minute black spots. These dusky features seemed to suggest an analogy to Mount Rossa, on the southern flank of Etna, which, according to Scrope, completely covered the surrounding country with a deposit of black sand during an eruption in 1669. Possibly something of this kind might now be occurring on the moon, and certainly it would be amongst the smaller craters that we should look for evidence of present volcanic activity.

The President said he had, at Dr. Spitta's request, lately tested his new occulting eye-piece. It might briefly be described as a Ramsden eye-piece, fitted with two pairs of moveable shutters at the focus. Its work on double stars was excellent, especially when the companion happened to be faint and not too close, and he had by its aid discovered a new companion to 27 Hydræ. This was a wide and lovely pair, *s. p. a.* Hydræ, and was noted by the elder Herschel, though he did not think he had discovered its triplicity. The pair was of 6·0 and 7·0 mag., but, on examining them with this eye-piece, a small comet of 11 mag. (Struve's scale) was at once detected about 8″ from the 7·0 mag. star, and at about 190°. Altogether, he thought the

eye-piece supplied a long felt want, but, from the nature of the eye-piece, there was an unfortunate liability to "ghosts," hence there was a danger of finding companions which had no existence.

Mr. J. E. Gore, F.R.A.S., contributed a series of observations of the remarkable variable *Mira* Ceti. According to Argilanders formula, a maximum was due on January 28th; but from his own observations this was not attained until February 4, when he estimated it equal to *A* Ceti, or about 2·7 mag. The increase of light was at first very rapid, but it became afterwards much slower, and seemed to remain constant from Feb. 4 to Feb. 13.

Mr. R. Wilding, hon. curator of Preston Observatory, announced his discovery of Tempel's comet. Its position was R.A. 12h, and about 19° N. dec. The nucleus appeared like a 13th mag. star surrounded by a nebulous disc. The Secretary gave notice that he would bring forward for discussion at the next meeting, a motion to enable members who reside at a distance to vote at the Annual General Meeting by proxy.

Eleven gentlemen were elected members, and Dr. Copeland, of Dun Echt, was elected an associate of the Society.

SELENOGRAPHICAL NOTES, MAY, 1885.

By Thos. Gwyn Elger, F.R.A.S.

The Region between Copernicus and Gambart.—Dr. Klein has recently called attention, in the *Journal* of the Liverpool Astronomical Society, to the very minute and enigmatical objects in the neighbourhood of Gambart, discovered by Schmidt, and specially alluded to in the quarto volume accompanying his great lunar chart, though they were found far too delicate and evanescent to be accurately recorded in their proper positions in the map. Requiring unusually favourable atmospherical conditions and high powers, and being moreover invisible except under a very high angle of illumination, when the shadows of neighbouring objects have ceased to exist, it is only on very rare occasions that they can be searched for with any hopes of success. In January last, however, Dr. Klein succeeded in obtaining very satisfactory observations of portions of the district which includes them, and he has propounded a very feasible hypothesis to explain the black colour they assume under a nearly vertical sun. No existing lunar map will compare as to detail with that of Schmidt's in the representation of the remarkable country included by lat. 0° and N. lat. 15°, and E. long. 10° and E. long. 25°. It is only by carefully studying his delineation of it that we can obtain a just idea of its true character. In Section VI. he shows a complex

hill-system, extending from the north of Gambart in a north-west direction, and passing about twenty miles on the east of Gambart C. Another somewhat shorter but broader range runs from near the foot of the southern glacis of Copernicus in the direction of Gambart, but terminates before reaching it, the two ranges including a V-shaped area of some 5,000 square miles, with its apex directed towards the south. Associated with the first or more westerly of these hill-systems, and lying mostly on the south and east of Gambart C, is an incredible number of craterlets of the smallest type, many not exceeding a few hundred feet in diameter, the district, in the words of Dr. Klein, being "perforated like a sieve." It was here, on the night of January 25th, that he was able to detect some of the most remarkable of the objects to which Schmidt alludes. One of them was not more than $\frac{1}{3}''$ in diameter, and appeared at times of best definition to be perfectly black. In its neighbourhood was a small double crater, with a black border, resembling the dark craters observed by Schmidt in the Mare Nectaris and elsewhere. South of Copernicus, and lying a little west of a line passing from Reinhold A and the double crater Copernicus A, A^1, is a curious double craterlet, marked *m* in Schmidt's Sect. VI, but omitted by Mädler and Neison. This object shows a bright white interior, while its slopes are covered with dark material, which gives it the appearance of being surrounded by a grey nimbus. One may imagine that Mt. Rossi, a parasitic double cone on the southern slope of Etna, would present from the moon a very similar appearance after its formation in 1669, when, according to Scrope, the ejecta produced a deep deposit of black sand, which covered a circuit of about two miles radius. Copernicus *m*, however, is a giant to the minute black points of Dr. Klein, which seem to be allied to crater-pits, and whose dark colour he suggests may probably be caused by the *débris* of recent eruptions. There are two well known circular black spots of considerable size, visible at and about full moon, amid the intricacies of the south wall of Copernicus, after all other shadows have disappeared, which are difficult to account for except on a similar supposition to that advanced to explain the infinitely smaller black points near Gambart C. The larger and better defined of these features is always so distinct in good observing weather at full moon, that it is strange that no notice of it is taken by Neison and other Selenographers.

It is to be hoped that Dr. Klein's interesting communication may induce those who command powerful telescopes to turn their attention to the minutiæ to which he draws attention, which are, of course, hopelessly beyond the power of small

apertures. It is amongst objects of this class that evidence of present lunar activity is to be looked for with the greatest probability of success. But the first requirement of the investigation is a carefully prepared chart to a very large scale of the area in question, on which every feature visible with high powers, and under various angles of illumination, may be set down and subsequently described and catalogued. Any investigation less rigorous than this will only end, as in the case of Linné, in a cloud of scepticism and uncertainty.

Encke.—On 25th February, from 8h. to 9h., this ring-plain was well placed for observation, the east longitude of the morning terminator being about 45°. At this stage of illumination the hexagonal form of the bright low border is very obvious, and the small craterlet on the eastern wall a conspicuous feature. The interior of Encke is traversed from north to south by two parallel ridges. The principal ridge commences at the south-east border, and divides the formation into two nearly equal portions. A little north of the centre it suddenly increases in width and height, forming a low central mountain, and then divides into two branches, one branch running to the north-west, and the other to the north-east border, the space included between these arms and the north-west and north-east walls being notably darker than the remainder of the floor. The second ridge, lower and less distinct than the last, lies between it and the eastern border, and joins the eastern branch of the central ridge near the inner foot of the north-eastern wall. On the south-west side of the floor, close to the border, a long light streak was noted, which probably represents a ridge. Schmidt's drawing, though full of detail, does not show the bifurcation of the principal ridge, while Neison, following Mädler, depicts the ridge as terminating on the north at a central mountain. On 27th February, the east longitude of the morning terminator being about 69°, Encke and its surroundings were to a great extent involved in the bright streaks radiating from Kepler, and it was only with difficulty that the outline of the western border could be made out. The crater on the south wall, discovered by Schmidt, (which was not seen on the 25th), was visible as a very bright spot, while that on the eastern wall could not be traced. The only objects discernible in the interior were a brilliant strip near the north-east border, evidently a portion of the secondary ridge just referred to, and an equally bright spot near the south-west wall, which agreed in position with the fainter streak observed on the 25th. On the Mare, between Encke and Kunowsky, are many craterlets not shown by Mädler or Neison, among them a conspicuous double craterlet due west of Encke, and about 30 miles distant from it.

Kempston, Beds: 20 April, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

Sir,—I shall feel indebted if you will allow me, through the medium of your columns, to ask any of your readers who possess observations of Jupiter's Satellites, appearing otherwise than normal, to forward the same to my address. Especially would I call attention to the following two queries. Have they ever seen Sat. IV. otherwise than black, chocolate, or steel-grey, during transit? or, Sat. II. otherwise than white, also during transit? Should any reader have any such observations unpublished, I shall be glad to be at the same time informed, where it is possible, the temperature at the time of the observation, the instrument used, whether refractor or reflector, its aperture and power employed.

EDMUND J. SPITTA.

Ivy House, Clapham Common, London, S.W.

ANOTHER RED SPOT ON JUPITER.

There is now no doubt about the character of the mark following the great red spot to which reference was made in last number of the Register, it is evidently a small one of the same kind. It was observed this evening under favourable circumstances. The old red on the meridian about 7·30, and the new red following about 9.0, so there is 1h. 30m. between them; and the new spot is best seen when the old one is disappearing at the limb. The position of the new spot is on the polar edge of the southern belt, and its size about half the length by one third the breadth of its forerunner.

NATHANIEL E. GREEN.

39, Circus Road, St. John's Wood:
April 20, 1885.

DOUBLE STARS WITH LARGE PROPER MOTIONS.

The following eight double stars are remarkable for the large proper motions of the principal stars, in which the companions in each case share; the distance of the small satellite in every

one of the eight cases being within 17″ and above 7″, the colour of the companion being usually blue, and that of the primary yellow; and the difference of magnitude between the large star and the small one very marked. The proper motions, taken from Mädler, are for 100 years, and the magnitudes expressed according to Σ's scale. The measures are the most recent ones.

Star	*Proper Motion.*	*Magnitudes.*	*Pos. Angle.*	*Distance.*	
5 Serpentis ...	58″·9 in 155°·0	5·0, 10·5	38°·0	10″·7	Slight increase in distance.
ξ Pegasi ...	52·6 in 159·1	4·6, 12·0	112·6	11·9	Binary.
ι Ursæ Maj. ...	52·5 in 238·9	3·2, 10·5	355·9	9·4	Binary.
39 Leonis ...	49·8 in 261·8	6·0, 11·5	298·9	7·1	Fixed. Companion variable.
β Aquilæ ...	48·9 in 172·0	3·7, 12·0	17·2	12·2	Fixed.
τ Boötis ...	44·4 in 275·3	4·1, 11·5	354·2	8·9	Binary. Companion variable?
θ Persei ...	35·0 in 107·4	4·2, 10·0	299·0	16·5	Binary?
υ Ursæ Maj....	33·8 in 233·7	3 9, 12·5	294·4	11·5	Fixed.

The $4\frac{1}{2}$ mag. star *n* Lyncis (10 Ursæ Maj. of some catalogues) has precisely the same proper motion as ι Ursæ Maj., though some 7° distant from it. It will be remembered that ι Ursæ Maj. has, according to Peters, a parallax of 0″133 ($25\frac{1}{3}$ light years).

H. SADLER.

THE DOUBLE STARS 94 CETI, 95 CETI, AND ξ PEGASI.

The three pairs 94, 95, Ceti, and ξ Pegasi, though not included in Messrs. Crossley, Wilson, and Gledhill's, or other catalogues of binary stars, are yet well worthy of more attention from observers than they have hitherto received, as the faint companions in each case participate in the large proper motion of the bright stars, and in two instances afford evidence of motion of a binary character also. 94 Ceti is entered in the third catalogue of Sir John Herschel, having a companion rated by him at the 19th magnitude at an estimated position angle of 255° and distance 6″ (1828). He calls it a most delicate object, but never measured or observed it again. Smyth perceived the companion in the autumn of 1836, "the night being a perfect one" (he failed, however, he says, on the same night to see the companion of 30 Eredani; 17 mag. according to Herschel, but only 12 mag. in H.'s scale according to Dembowski, and rated

even brighter by Winnecke) and estimated P=260°, D=5″; but the only recorded *measures* are the following:

	°	″				
De.	253·3	5·19	1n	1865·932	11·0 mag. "molto difficile"	7½-in. Merz
"	253·0	5·00	1n	1876·088	12·0 mag. "difficilissima"	"
β	250·9	5·73	1n	1877·698		18½-in. Clark
β	252·9	[3·28]	1n	1879·681	12·5 mag. "difficult"	6-in. Clark

De's and β's magnitudes are expressed in Σ's scale; β's distance in 1879, measured at Mt. Hamilton, Cal., at an altitude of 4,250 feet above the sea, is too small, owing to the inferior micrometer used. If the companion had been at rest its position in 1877 would have been at 269°·5 12″·7 from the principal star, if Herschel's estimates in 1828 were fairly correct. Both stars are therefore moving together through space with little or no alteration in their relative positions. The *c.p.m.* is 0′·25 annually in the direction of 111°.

The companion of 95 Ceti was discovered by Aloan Clark, in 1853, with a 7½-in. object glass of his own construction, and the pair was measured by Dawes with the same glass on three evenings in the autumn of the following year. It has never been seen double since, though frequently looked for; β found it single in 1878 with a power of 925 on the 18½-in. refractor at Chicago. Dawes gave from the three nights' measures in 1854,

73°·2 0″·73 *est* 3n 1854·81 5½, 10 mags. A yellow, B purplish (?).

This is evidently a binary pair, for if the small star had been fixed it would have been 5″·5 distant from A in the direction of 296° at the time of Burnham's observation.

ξ Pegasi is entered as No. 301 in Sir John Herschel's first catalogue, as having an 18 mag. companion of a distinctly blue colour at 120°±: 11″±. Shortly after this discovery the star was measured by Struve, and afterwards by Smyth, who noted a very distant companion, which, if his measures can be trusted, has no connection with the binary pair. For the close pair we have:

	°	″			
Σ.	119·6	12·41	1n	1828·68	B 12·0 mag.
H.	122·3	13·5±	2n	1830·90	
De	117·7	12·17	2n	1866·79	12·0 mag.
β	112·6	11·93	4n	1879·38	12·4 mag.

The magnitudes are in Σ's scale. Dawes rated it 11·2 of this scale in 1857; H 12·13 mag. of *his* scale in 1830. A has an annual *p.m.* of 0″·526 in 159°·1 (Mädler), in which the small star evidently shares, and at the same time appears to be slowly revolving round the large star in a retrograde direction.

H. SADLER.

OBSERVATIONS OF THE FIRST CONTACT OF THE PARTIAL SOLAR ECLIPSE OF MARCH 15-16, 1885, *maue at the U.S. Naval Observatory, Washington.*

(Communicated by Rear-Admiral S. R. Franklin, U.S. Navy, Superintendent).

Washington Mean Time.	Observer.	Remarks.
March 15. h. m. s.		
23 57 6·9	H.	Observation fair, perhaps one or two seconds late. The instrument was the 26-inch Equatorial, with aperture reduced to 4 inches and power 383.
23 57 4·7	F.	Observed with 9·6-in. Equatorial, with aperture reduced to 4 inches and power 90. Observation satisfactory.
23 57 7·5	P.	Aperture 5 inches, magnifying power about 90. Do not think I could have observed it one second earlier with the state of the atmosphere as it was.

No observations of the last contact could be made on account of clouds.

The observers are Professor A. Hall, Professor E. Frisby, and Assistant Astronomer H. M. Paul.

The difference of right ascension of the second limbs of the moon and sun was observed by Lieutenant C. G. Bowman on 11 wires of the East Transit Instrument, and by Assistant Astronomer W. C. Winlock on 9 wires of the Transit Circle. The moon's limb was faint at times, owing to flying clouds. The sun's limb was but moderately steady.

RESULTS.

B., Diff. 1m. 56·49s. ± ·028 seconds.
W., 1m. 56·25s. ± ·031 „

Ninety-nine photographs of the various stages of the eclipse were taken with the photo-heliographic apparatus of the Transit of Venus Commission by a party consisting of Commander Brown, Ensigns Taylor and Winterhalter, and Mr. W. F. Gardner, the instrument maker of the Observatory. The exposures were made by Ensign Winterhalter.

REVIEWS.

Stonyhurst College Observatory. Results of Meteorological and Magnetical Observations. By the Rev. S. J. Perry, S.J., F.R.S., &c. 1883. Manresa Press, Roehampton. 1884. pp. 93.

An appendix contains the results of meteorological observations taken at the newly-founded observatory at St. Ignatius' College, Malta, from June 1 to Dec. 31, 1883. Stonyhurst College Observatory is in communication with (besides other persons and places) the central meteorological office in London, the chief signal officer of the U.S.A. the Registrar General, the Sociétê Météorologique de France, the Upsala Observatory,

Mr. J. Symons. In astronomy, sun drawings on the scale of 10½ in. to the diameter, measures of the chromosphere and the spot-spectra observations are taken daily and communicated to the R.A.S., and other quarters, as also are observations of Jupiter's satellites, occultations of stars, and meteor-streams. The following particulars are of interest:—The velocity of the wind (63 miles per hour) at 1 a.m. on the 12th of December, 1883, was the greatest ever recorded. The unusual glow at sunrise and sunset first attracted attention on November 26, 1883, and was carefully watched ever since. The epoch of maximum intensity was December 16, 1883, and then the glow was visible for 1h. 50m. after sunset. On the coincidence of auroræ with solar outbursts and magnetic storms, the result of a careful examination of the observations of 1883 indicates that auroral displays coincide in every instance with periods of solar disturbance, and there have as yet been no auroræ remarked during periods of solar quiet. Similarly in every case of aurora the magnets were disturbed, although in two instances not violently. The displacement of the bright lines in the spectrum of the chromosphere favour the supposed connection between auroræ and solar disturbance. There is also some evidence to show that the auroræ and magnetic storms synchronise rather with particular classes of spots than with solar disturbances generally. We seldom see so much valuable and interesting information in so small a compass, and so agreeably presented, as in these little annual volumes from Stonyhurst Observatory.

Meteorological Observations made at the Adelaide Observatory and other places in South Australia and the Northern Territory during the year 1881, under the direction of Charles Todd, C.M.G., F.R.A.S. Observer, Postmaster-General, and Superintendent of Telegraphs. Published by authority of the Government of South Australia. Adelaide, 1884. Pp. xlix, 300.

This large and well-printed volume, with its maps, diagrams, and many tables, shows even above former years, the exhaustive way in which meteorology is cultivated at our antipodes, to the future great benefit, we may hope, of agriculture and commerce. At the invitation of the Victorian Government, a second Meteorological Conference was held at Melbourne, in April 1881, at which the following delegates were present:—James Hector, Esq., M.D., C.M.G., F.R.S., Inspector of Meteorological Stations, New Zealand; H. C. Russell, Esq., B.A., F.R.A.S., Government Astronomer, New South Wales; R. L. J. Ellery, Esq., F.R.S., F.R.A.S., Government Astronomer, Victoria; C. Todd, Esq., C.M.G., F.R.A.S., Government Astronomer, South Australia. The previous conference was held at Sydney in November, 1879, at which many important resolutions were agreed to for the extension and improvement of the organization of the work, some of which have been carried out. The chief stations are Melbourne, Sydney, Wellington, and Adelaide. There are fourteen out-stations, abstracts of the observations of which appear in Appendix III. The number of rainfall stations is 170. Valuable meteorological observations are received from Fiji. The early co-operation of Tasmania is hoped for, and that of Queensland is much desired. Appendix I. is an abtract or summary of the observations for 1881. Appendix II. contains observations of the eclipses, occultations, and transits of Jupiter's satellites, notes on the physical appearance of Jupiter and the transit of Mercury, which was observed at Adelaide Observatory by Mr. Todd with the 8-in. equatorial (Cooke), and by Mr. Ringwood with a 4½ in. equatorial. The times as noted by both observers agree remarkably well

to a very few seconds. During the occult.-disappearance of the second satellite of Jupiter, January 24th, 1881, the whole of the satellite could be distinctly seen through the limb of the planet up to the time of disappearance.

On the Amount of Atmospheric Absorption. By S. C. Langley. [From the *American Journal of Science*, Vol. xxviii. Sept. 1884.] pp. 18.

The amount of atmospheric absorption at the sea level is generally taken as about 20 per cent. of the whole. The investigations of Prof. Langley, which are still incomplete, tend to show that it is really more than twice that amount. At the conclusion of this paper the Professor says, "Roughly speaking, we may say that we receive on the average at the sea level as much light from the sky as we do from the sun itself; getting more light from the sun at mid-day than from the sky, but more in the morning and afternoon from the sky than from the sun. . . . All my own observations, whether through observations at the sea level, or at an altitude of nearly 15,000 feet, lead me to believe it probable that the mean absórption of light (and of heat also) by our atmosphere is *at least double* that which is customarily estimated, and also to conclude that fine dust particles, both near the surface and at a great altitude, play a more important part in this absorption, both general and selective, than has been heretofore supposed."

THE PLANETS FOR MAY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	2 12 20	N.12 55½	11″·8	23 30.2
	9th	2 4 24	N.10 7½	11″·0	22 50·8
	17th	2 12 58	N. 9 46½	9″·4	22 27·9
	25th	2 36 55	N.11 41½	8″·0	22 20·3
	1st	2 37 45	N.14 33	9″·6	23 55·5
Venus ...	9th	3 11 51	N.17 16	9″·6	0 2·0
	17th	3 51 57	N.19 55½	9″·6	0 10·5
	25th	4 33 13	N.22 0½	9″·6	0 20·2
Jupiter ...	1st	9 54 35	N.13 58	36″·4	7 15·1
	9th	9 55 51	N.13 50	35″·6	6 44·9
	17th	9 57 49	N.13 38½	34″·8	6 15·4
	25th	10 0 26	N.13 23	34″·0	5 46·6
Uranus ...	1st	11 58 42	N. 0 58½	4″·0	9 18·9
	17th	11 57 22	N. 1 6½	3″·8	8 14·6

Mercury rises about 10 minutes before the sun at the beginning of the month, the interval gradually increasing.

Venus sets just after the sun on the 1st, the interval slightly increasing.

Jupiter sets about 2 hours before sunrise at the beginning of the month, and afterwards earlier each night.

ASTRONOMICAL OCCURRENCES FOR MAY, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. αCoronæ
Fri	1	15 14 16 22	Occultation of 29 Ophiuchi (6) Reappearance of ditto	2nd Tr. I. 2nd Sh. I.	11 5 13 33	12 49·4
Sat	2		Sidereal Time at Mean Noon 2h. 42m. 15·34s.	3rd Tr. E. 3rd Sh. I.	11 43 13 5	12 45·5
Sun	3		Sun's Meridian Passage 3m. 17·99s. before Mean Noon	2nd Ec. R.	11 35 17	12 41·5
Mon	4	5	Superior Conjunction of Venus and Sun	1st Oc. D.	14 3	12 37·6
Tues	5			1st Tr. I. 4th Sh. I. 1st Sh. I.	11 24 11 37 12 39	1 33·7
Wed	6	20 43	☾ Moon's Last Quarter	1st Oc. D. 1st Ec. R.	8 32 12 4	12 29·7
Thur	7			1st Sh. E.	9 27	12 25·8
Fri	8		Saturn's Ring: Major axis=38"·12 Minor axis=17"·34	2nd Tr. I.	13 38	12 21·9
Sat	9			3rd Tr. I.	11 56	12 17·9
Sun	10	15 27	Near approach of 44 Piscium (6)	2nd Oc. D.	8 46	12 14·0
Mon	11		Conjunction of Neptune and Venus 1° 15′ N.			12 10·1
Tues	12	16 16 21	Conjunction of Moon and Mercury 0° 21′ S. Conjunction of Moon and Mars 2° 4′ N. Conjunction of Mars and Mercury 2° 27′ S.	2nd Sh. E. 1st Tr. I.	8 23 13 17	12 6·1
Wed	13	4	Conjunction of Neptune and Sun	4th Oc. D. 1st Oc. D. 3rd Ec. R.	9 58 10 25 10 35 42	12 2·2
Thur	14	3 17 6	● New Moon Conjunction of Moon and Venus 3° 47′ N.	1st Sh. I. 1st Tr. E. 1st Sh. E.	9 3 10 6 11 22	11 58·3
Fri	15		Illuminated portion of disc of Venus= 0·999 Illuminated portion of disc of Mars=0·985	1st Ec. R.	8 27 49	11 54·3

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. αCoronæ —
Sat	16	6 31 7 27 3	Occultation of 130 Tauri (6) Reappearance of ditto Conjunction of Moon and Saturn 4° 2′ N.			11 50·4
Sun	17	3	Jupiter at quadration with the Sun Sidereal Time at Mean Noon 3h. 41m. 23·66s.	2nd Oc. D.	11 21	11 46·5
Mon	18		Sun's Meridian Passage 3m. 47·07s. before Mean Noon			11 42·5
Tues	19	10 42 11 15	Occultation of α Cancri (4) Reappearance of ditto	2nd Sh. E.	10 59	11 38·6
Wed	20	17 45 12 21 15	☽ Moon's First Quarter Occultation of B.A.C. 3407 (6) Conjunction of Moon and Jupiter 4° 17′ N.	3rd Oc. R. 3rd Ec. D.	9 33 11 10 36	11 34·7
Thur	21	8 48 9 18	Occultation of 35 Saxtantis (6) Reappearance of ditto	1st Tr. I. 1st Sh. I. 1st Tr. E.	9 41 10 57 12 1	11 30·7
Fri	22			4th Sh. E. 1st Ec. R.	10 4 10 22 55	11 26·8
Sat	23					11 22·4
Sun	24					11 18·9
Mon	25					11 15·0
Tues	26			2nd Sh. I. 2nd Tr. E.	10 41 11 6	11 11·1
Wed	27	14 10 15 5	Occultation of θ Libræ (4½) Reappearance of ditto	3rd Oc. D.	9 57	11 7·2
Thur	28	8 30	○ Full Moon Saturn's Ring: Major axis=37″·56 Minor axis=17″·03	2nd Ec. R. 1st Tr. I.	8 39 13 11 37	11 3·2
Fri	29			1st Oc. D. 1st Ec. R.	8 46 12 18 3	10 59·3
Sat	30	9	Conjunction of Mars and Mercury 3° 15′ S.	1st Sh. E.	9 40	10 55·4
Sun	31			3rd Sh. E.	8 36	10 51·4

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
May 1	+60 41 E.	May 11	—61 24 E.	May 21	— 3 33 M.
2	48 28	12	73 36	22	15 45
3	36 16	13	85 50	23	27 58
4	24 4	14	+81 57 M.	24	40 11
5	11 51	15	69 45	25	52 24
6	— 0 21 E.	16	57 32	26	64 38
7	12 34	17	45 19	27	76 50
8	24 47	18	33 6	28	89 3
9	36 59	19	20 53	29	+78 44 E.
10	49 12	20	8 41	30	66 31
				31	54 17

Moon farthest from the Earth, May 3, 22h.
„ nearest to „ „ „ 15, 22h.
„ farthest from „ „ „ 31, 12h.

Books received:—Sidereal Messenger.—Meteorological Observations made at Adelaide during 1882.—Science Monthly.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1884.	To Dec., 1885.
H. M. Hollis.	E. Jones.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to John C. Jackson, Chief Office, London.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by Penny postage stamps or otherwise. Subscribers in America may remit, either by post-office order or in notes, 3½ dollars, in payment of one year's subscription, postage included.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11. *Angel Court, Throgmorton Street, E.C.,* **not later than the 20th of the Month.**

The Astronomical Register.

No. 270. JUNE. 1885.

ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

The third meeting of the session was held at the Society's rooms on Friday, May the 8th, 1885.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut.-Col. Tupman.

The following gentleman was balloted for, and duly elected a Fellow of the Society:

Professor C. H. Brewett Taylor, of Foo-Chou Arsenal, China.

Colonel Tupman read the minutes of the last meeting, and announced that 64 presents had been received by the Society since the April meeting, amongst them was a volume of "Photometric Observations," made by Professor Pickering and his assistants at Harvard with the meridian photometer. Mr. Ryle had presented to the Society a speculum metal reflector, which is believed to have been made by Sir William Herschel. It formerly belonged to Mr. Dawes, and was purchased by Mr. Ryle at Mr. Dawes' sale.

Mr. Knobel read a paper from Mr. R. A. Proctor on *Five Orders of Meteor Streams or Comets.* Mr. Proctor referred to the importance of Mr. Denning's discovery, which he supposed might be taken as now established, that certain meteor streams radiate for months together from the same points on the star sphere. Five years ago, before Mr. Denning's discovery was established, he doubted whether the discovery could be real,

because the inferences were so startling. That discovery bears but one interpretation—viz., that the motion of the earth in its orbit round the sun is almost as rest, compared with the velocity of these meteors. Without assuming that the radiants could be determined with the precision claimed by Mr. Denning, it was certain that the meteors from the long-lasting radiants travelled at the rate of hundreds of miles per second. The meteor streams which are observed for short periods year after year must be very different from the meteors of the long-lasting radiants; we must reject the speculation of Signor Schiaparelli, that these flights of scattered bodies were drawn to the solar system by the sun's attraction, and are travelling in closed orbits. Mr. Denning's discovery involved the flight of these meteors across the interstellar depths, and they must look for the source of the tremendous velocities of these bodies to the giant suns. In Mr. Denning's discovery, so far back as 1873, Mr. Proctor recognised three distinct orders of comets—viz., First, those ejected from the giant planets when in the sun-like state; secondly, those travelling in more widely extended paths, and probably ejected from the sun itself, but subsequently perturbed by planetary attraction; and thirdly, those whose paths show that they were originally ejected from suns like ours. To these two other orders must be added—viz., those whose paths show that they were originally ejected from our earth when in a sun-like state, and the remaining order would be those whose enormous velocities show that they were originally ejected, not from suns like our own, but from giant suns like Sirius and others.

Mr. Ranyard: There is one point I should like to mention. Matter thrown out from a giant sun, or from any star, would have the proper motion of that sun or star, as well as the motion with which it was projected. We do not know what these proper motions amount to. The proper motion, for example, of 1830 Groombridge, in the Great Bear, must be something enormous. It has very little parallax, so that the star must be moving with an actual motion of hundreds of miles per second, and this would be added to any velocity with which matter was thrown out, even if we do not suppose the matter to come from the giant suns that Mr. Proctor has spoken of. The proper motion alone of the stars may account for an enormous velocity. To account for Mr. Denning's observations the velocity must be incredible. If there is a shift of the radiant amongst the stars of one degree it would mean that the motion with which the meteors meet our atmosphere must be 53 times as great as our own motion, and much greater than anything we are accustomed to in the solar system.

Colonel Tupman: We must all admire the fertility of Mr. Proctor's speculations; but there occurs to me a serious objection to this particular speculation, and that is, that it is an undoubted fact that meteors have not such great velocities. I have found the velocity was sensibly parabolic in all cases, and I doubt whether any of them have a velocity of their own exceeding half as much again as that of the earth.

Mr. Ranyard: I should like to ask whether Colonel Tupman refers to ordinary meteors or to boloids that happen to have been observed from several places. Can we say that the velocity has been observed of any of these small meteors which come from fixed radiants? Their velocity is stopped in the higher atmosphere, and without simultaneous observations we cannot tell what the velocity is.

Colonel Tupman: We have a certain amount of evidence. We have the average angular velocities of meteors, from different radiants which may be assumed to be at the same height, and the average angular velocity is pretty much the same. There are certain radiants from which slow moving meteors come, but the majority seem to me to have approximately similar angular velocities. At 90 degrees from the radiant the average angular velocities appear to be about the same. If there were any double or treble velocities, the fact would become evident at once to an observer of experience. But the speed is never particularly fast, for you can follow them with your eye, and obtain an idea of the relative velocities. As a matter of fact, the mean angular velocity, when cleared of aberration, is essentially parabolic for all systems.

Mr. T. K. Mellor: I might suggest that it depends very much upon the part of the star from which the meteor is expelled what the motion would be. If the meteor were expelled in rear of the direction in which the star was going, the meteor would have the motion of the star *minus* the motion with which it was ejected, so that it does not follow that the meteor ejected would have the same motion as the star itself has.

Thanks were voted to Mr. Proctor for his paper.

Mr. Stone explained a paper on the right ascensions of the Cape Catalogues. Mr. Downing, he said, had carefully compared the Cape Catalogue of 1880 with the Melbourne Catalogue of 1870, and as the epoch only differed by 10 years, and approximate proper motions could be determined, such a comparison might give some evidence for or against the existence of systematic errors. The comparison instituted by Mr. Downing showed a close agreement between the Cape Catalogue and that of Melbourne; but when attempts were made to compare the Cape

Catalogues of 1850, 1860, and 1880, it is impossible to bring up the places for 20 or 30 years with sufficient accuracy to justify the assertion that the differences are due to systematic errors in the catalogues. He had pointed out in this paper that if they brought up the places with sufficient accuracy for 1850 and 1860 to 1880, to infer that the differences in right ascensions are due to instrumental defects in the 1880 catalogue, the result of these comparisons would show that the southern R.As. of the 1880 catalogue are *too small* relatively to those near the equator from a comparison with the 1850 catalogue, whilst the comparison with the 1860 catalogue would appear to show that these same R.As. are relatively *too large*. It was quite certain that one or the other conclusion must be wrong, and that these differences are not due to systematic errors in the 1880 catalogue. He did not think that the right ascensions of the 1850 catalogue were comparable to those of the 1860 or 1880 catalogues. A great number of the southern stars in the 1860 catalogue are contained in the Melbourne Catalogue for 1870, and there would be found in the latter catalogue approximate proper motions quite independent of the Cape Catalogues. A comparison between the 1860 and 1880 catalogues, using these proper motions, reduced the discordances between these two catalogues to such quantities as 0·05s., and they would be further reduced if Powalky's re-reduction of Lacaille's observations had been used for the determination of these proper motions. The proper motions deduced from the 1840 and 1880 catalogues, used as a test for the 1860 catalogue, gave differences of only 0·03s.

Mr. Downing was glad to find that Mr. Stone had been able to make some use of his comparisons of the Melbourne and Cape Catalogues. In one point he could hardly agree with Mr. Stone, viz., that the comparisons were not trustworthy on account of the inaccuracies of the adopted proper motions. In the comparison of the 1850 and 1880 catalogues there were no less than 3,368 stars used, and with such a large number as that the errors of the assumed proper motions *plus* and *minus* would be about equal, and would not materially affect the result. In the comparison of the 1840 and 1850 catalogues there were 1,090 stars' places used, and the result there was not likely to be affected. In the case of the Melbourne Catalogue of 1870 and the Cape Catalogue of 1860 there were more than 900 stars used. When they took stars over several degrees of declination throughout the 24 hours, it could not be assumed that there was any decided drift. He thought the right ascensions of the Cape Catalogue of 1880 were superior to those of the Cape Catalogue of 1850, and on that point probably Mr. Stone would agree with him.

Mr. Stone replied that, of course, if they took a large number of proper motions the errors might eliminate themselves, but he thought that would not apply to the cases he had been speaking of, and the comparisons made, whenever materials existed for the determination of proper motions, independently of the catalogues, prove this to be the case. If any one would examine the proper motions of southern stars for the first four hours, they would find an extraordinary difference in the *plus* proper motions, and it would require a large number of observations indeed to effectually eliminate the differences. 20 years was a long time, and when they divided $\frac{3}{100}$ths by 20 it came to a very small quantity.

The thanks of the Society were voted to Mr. Stone for his paper.

Mr. Knobel read a paper by Mr. John Maquire on total solar eclipses visible in the British Isles from 878 to 1724. The paper enumerated those eclipses the central line of which had touched the British Isles, and gave tables showing the width of the shadow and the central lines. London had been twice visited by total eclipses (in 878 and 1715), Dublin twice, and Edinburgh five times. The moon's shadow had fallen, during the period named, on every spot in the British Isles except a small space on the coast of Ireland.

The thanks of the Society were voted to the author of the paper.

Colonel Tupman read a paper by Mr. W. F. Denning on a peculiar variety of meteors observed on the morning of April 20th. As Mr. Denning was watching the progress of the Lyrids he saw a remarkable meteor projected apparently on the stars E. and S. Lyra. It rose upwards with great rapidity. It was a meteor of the third magnitude, and its peculiarity was its marvellous velocity. It appeared to be in the air a few yards distant only from the observer. It must have traversed over 16 degrees on the background of the sky in the $\frac{1}{20}$th part of a second, but of course there was great difficulty in estimating such short intervals of time.

The thanks of the Society were voted to Mr. Denning for his paper.

Special Meeting.

At nine o'clock Colonel Tupman read the notice convening the meeting, and the proposed resolutions.

Lord Crawford, in moving the first resolution, said: We are very limited for time this evening, and therefore I propose to be very short. I have brought this matter forward as it is one which has been occupying certain members of the Society for a

number of years. It has been felt that those Fellows of the Society who do not live in or near London are, to a certain extent, under disability in not being able to vote in electing the President and Council of the Society. It seems to me that the accident of their living at a distance from the metropolis should not disqualify them from taking a part, if they desire to do so, in selecting the governing body of the Society, whose action may have an important influence on the welfare of astronomy. I am in no way affected myself by this disability which lies on our country and foreign Fellows, for I usually manage to be present at our elections. I therefore feel that I am not actuated by any personal motive in proposing this alteration of the bye-laws. I am only actuated by a desire for the benefit of the Society, and a wish that our country Fellows, of whom we now have a great number, should take a greater interest in the affairs and action of the Society. Though great attention has been given to the wording of the resolutions, I now see that there is an error in the first resolution, from which it might be supposed that a Fellow might hand in more than one list. It should run, "A list," not "lists," "handed to the scrutineers of the ballot on behalf of any Fellow not present at the meeting shall be accepted, if duly verified by the signature of the absent Fellow." It is proposed to add these words at the end of bye-law 12.

Mr. Mattieu Williams seconded the motion.

Mr. Brett said that Lord Crawford had made one slight mistake. He said members were disqualified, but they entered the Society well understanding that they were not to vote unless they were present at the meeting. It was therefore hardly correct to say they were disqualified.

Lord Crawford: Debarred, then.

The President: Before the discussion proceeds any further I think it may be as well for me, as President of the Society, to make one or two remarks showing the position the Council have taken up in regard to this question. The Council generally have great sympathy with the object of this motion, and they are desirous in every respect that something to its effect shall be passed by you to-night. (Hear, hear.) At the same time, in stating this I should say that they would not on any account sanction a resolution of this kind unless certain precautions were taken so as to prevent any irregularity in the voting. (Hear, hear.) The Council are prepared to agree that a list may be forwarded by post in a closed envelope under cover of a letter signed by a Fellow, and addressed to the scrutineers of the ballot, and that list shall then be accepted by the scrutineers as the voting paper of that Fellow. There can be no

objection whatever to an absent Fellow voting in that manner. I am very glad to say there are many Fellows of this Society residing in all parts of England, Scotland, and Ireland, and also in foreign countries, and I believe most of those Fellows take a great interest in the Society, and that they are very desirous that they should have a voice in the election of its officers and Council. I and all my colleagues on the Council will agree with them that that wish is a proper one, but before acceding to it certain precautions must be taken. It would not do, in my opinion, that these country Fellows should send their lists to other Fellows, and that those Fellows should present them to the scrutineers without proper identification. There must be order in all these matters, or we shall come to trouble very soon. I have had some 40 years' experience in this Society, and therefore you will allow me to express my opinion that I believe we shall be doing the Society a great deal of harm unless we add to Lord Crawford's resolution some such words as I have suggested. The Council have every sympathy with this motion, if, in passing it, it is understood that a list handed to the scrutineers of the ballot on behalf of any Fellow shall only be accepted after being duly verified, and that it shall be sent in a formal manner to the Secretary of the Society, in some such way as that indicated by me, before 12 o'clock on the day of meeting, so that it may be duly verified by the Assistant-Secretary. I think that if Lord Crawford will accept the suggestions I have made the resolution might be almost passed unanimously.

Lord Crawford said that if he understood the President's proposal rightly, it was, that the scrutineers would be entitled to receive from any Fellow, a list conveying his desires with reference to the election of the Council, provided that the list was enclosed within a letter stating that desire, and received by the scrutineers through the post.

The President: Yes, that is so, after due verification.

Mr. Chambers said he ventured to think that a very large number of the Fellows of the Society would welcome the concession which the President, on the part of the Council, had suggested should be made. At the same time he hoped the noble Lord, who had brought forward this motion, would not consent to withdraw his proposal except on some very special and clear understanding that the Council would pledge itself to propose at an early meeting of the Society such consequential amendments, as in their judgment were necessary to improve the present bye-laws in the direction indicated by the motion now before the meeting. He stated this frankly, because he must confess there

were feelings of distrust of the Council. At the meeting of the Society in 1880 a requisition was handed in by various Fellows inviting the Council to adopt such measures as might seem to them expedient for a complete revision of the bye-laws. That was five years ago, and as he understood it, nothing had been done, and the first suggestion of something being done was under pressure of something stronger being done by the Fellows of the Society at large. He doubted whether the members of the Council realized the fact that the great mass of country Fellows felt themselves hopelessly disfranchised by the state of things which now existed. The fact was incontestable, that, instead of 500 Fellows taking part in the election of 15, it practically amounted to 15 taking part in the election of 15, and the result was that the Council of this Society did not command the unreserved confidence of the great mass of the Fellows of the Society. In evidence of that, he said that the Society did not attract to itself the important papers it used to attract 15 or 20 years ago. The cause of that was to be found in the want of confidence felt by many Fellows of the Society in the Council—he did not mean as at present constituted, but in the constitution of successive Councils for some years past, depending, as they had done, on virtually the self-elected principle. It was to change that—a change which he believed was called for in the interests of astronomy—that the present proposal was made.

Mr. Beck hoped that the change proposed would not be made. He felt strongly as regards the independence of the Fellows of the Society, and that this change would be throwing into the hands of the Council a power which it was undesirable to give. It might sometimes happen that it was desirable to remove a member from the Council, but if the proposal was adopted, there was no doubt the majority of the country members, when receiving those lists and not knowing the circumstances of the case, would vote for the Council's list, and there would therefore be a greater difficulty in making any alteration in the Council's list than there was at present. He believed they would be giving up their power of introducing a new name if they were to be overwhelmed by a large number of persons knowing nothing of the detailed circumstances of the case. He believed his views would be sympathised in by a large number of Fellows who attended the meetings of the Society, and he hoped no change would be made.

Sir Edmund Beckett said the proposal of the President seemed to him to be a very good one. It was a mere matter of machinery as to how absent members should be allowed to vote. He could hardly understand how there could be any dissentients to the main

proposition. They should remember that in voting for the Council there was no debate, and there could be no debate about a person's character or fitness. He could see no reason against the proposition except the jealousy of those who lived in London against those who did not live in London. He thought there was force in what Lord Crawford had said, namely, that people were certain to take more interest in the Society if they felt they had more power in the election of those who administered its affairs. He was afraid he must also endorse what Mr. Chambers had said about the evident decline of interest in their proceedings. Look at the thinness of the numbers compared with what they used to be. Those, he thought, were good and practical reasons why, if they wanted to keep the Society flourishing, they should pass this resolution, and let people know that if they joined the Society they would have the same power of electing governors as if they lived in London.

Mr. Ranyard said he thought that before the motion was adopted the words which the President wished to have altered or added to the resolution should be definitely stated and put to the meeting. It was important that this matter should be disposed of then, and not left to the Council.

The President said that the list should be forwarded by post under cover of a letter signed by the Fellow, in order that the signature might be verified.

Mr. Ranyard thought there would be great advantages in not making more change in the motion, of which notice has been given, than was absolutely necessary, and if they could by a single word meet the wishes of those who dissented from the proposition as it now stood, it would be an advantage. He had no objection to see the words "posted to the scrutineers" added, but he thought there would not be any real advantage in that, as lists could be collected by any one who undertook to canvass in London, and then posted to the scrutineers. He did not see any advantage in taking out the words "duly verified by absent Fellows." The lists should, of course, be verified, and there should be no chance of any member handing in more than one. He saw no advantage in making a Fellow sign an envelope on the outside rather than the list within. There seemed to be an idea that if the lists were signed it would not be a voting by ballot. Any Fellow could vote secretly if he chose to by handing in his own paper, but if he chose to allow another Fellow to hand it in for him there was no penalty in the bye-laws for disclosing his vote. In fact, the votes would be continually recognised, although not signed by the Fellows, for the hand-writing of several Fellows was well-known. It was perfectly impossible to prevent people

allowing the scrutineers to know their votes if they thought fit. He thought it would be much simpler if they passed the resolution in the form in which it had been proposed.

Sir Edmund Beckett: You mean to preserve the right of people to hand in their own papers when they are here.

Lord Crawford: Yes.

Mr. Newall said he lived 300 miles from London, and he was deprived of voting when he should like to do so, owing to the distance from London at which he lived. He saw no objection to a voting paper being sent in an envelope, with the signature of the Fellow outside, and the scrutineers could open it and throw the paper into the voting box, and no one need know who gave the vote.

Captain Noble said, as to London Fellows voting, he wished to remark that he occasionally attended the scientific meetings of the Society, and met a number of gentlemen, but when he came to the meetings at which voting took place he found a totally different set of people. He did not think that the regular attendants at the ordinary meetings should be outvoted by those who only appeared at the business ones. He considered that the proposed alteration would put everybody on the same footing, and hoped that it would be adopted.

Dr. Schuster thought that no change should be made in the voting at present. The form of the amended proposal was only a little less objectionable than the first proposition, and he thought at any rate that they ought not to agree to it, except under the distinct understanding that the second paragraph should be withdrawn, for if they permitted voting by proxy on one question, they might permit it on all questions. The remarks of Captain Noble would lead them to the conclusion which he thought was a reasonable one, that it would be well to alter the time of the Annual Meeting, and have it in the evening instead of in the afternoon. One objection he had against the resolution was, that it would admit the principle which certainly ought not to be adopted, namely, that of allowing a man to give a vote without having heard the arguments on each side. Another objection was this. The Council list would be submitted, but it might be proposed to alter it, and he could not see how that could be done if the resolution was passed without objectionable discussions in newspapers on the merits of candidates. Members who wished to vote could easily get to London. Only that day he had delivered three lectures in Manchester, and his last one he had finished at four o'clock, and he then left to attend that meeting. Fellows present should not be led away by a mistaken form of kindness to those who were absent.

Mr. Stone said that so far as he was individually concerned his sympathies were entirely in favour of granting the power of voting to the Fellows generally. Individually it was not a question of hearing both sides of the question. He could not see that a man who came to the meeting and gave his vote, gave any more value to that vote than a man who sent the list from his home by post. The two propositions were practically the same, the only difference being, that the one brought forward by the President took certain precautions. It was necessary that a Fellow should send a letter stating that the voting paper enclosed in the envelope was his. The scrutineers could open the envelope, and be able to tick off his name as having voted, so that he could not attend the meeting and vote again, and his paper would be put into the box just as if he was in the room and put it into the box himself. He saw nothing revolutionary at all in that idea. He, however, held very different opinions with regard to the second resolution. In fact he was in favour of the first because he was opposed to the second. He wished the affairs of the Society to be managed by the Council, and he wished the Council to be in sympathy with the Society at large.

Mr. Knobel agreed with many of the remarks. He apprehended that the motion had been brought forward under the idea that a grievance and an evil existed. He believed that a grievance and an evil did exist, but it was due to the machinery of the bye-laws, which it had not been possible to obviate. The list was framed by the Council, and sent round to the Fellows, and the machinery which it was necessary to put in force to bring about any change in that list was very cumbersome, and it practically resulted in the Council's list being accepted. As to Mr. Chambers' remarks about the bye-laws, he rather wondered they had not been called over the coals for calling that meeting, because the bye-laws stated distinctly that notices of "Special General Meetings shall be only sent to the Fellows of the Society residing in or near London." He thought it would be an advantage if every Fellow belonging to the Society could be able to vote so as to take part in the ballot for the officers and Council. What had been suggested in the motion before the meeting was a distinct departure from other bye-laws, as the votes could be known, and then the election could not be said to be by ballot, which meant secret voting. The amended resolution that had been put forward by the Council would get over any difficulty the original motion might raise, and he very much hoped, and should be very glad, if the Council's suggestion could be accepted unanimously by the meeting.

Lord Crawford thought it would be very desirable for them

to accept the principal which the President had laid down in his proposal, and he would be glad to adopt it by amending his resolution: "Any list addressed and posted to the scrutineers of the ballot by any Fellow not present at the meeting shall be accepted if duly verified by the signature of the absent Fellow on the envelope."

Mr. Mattieu Williams said that, as seconder of the motion, he would accept the modification.

Mr. Brett said that no one was jealous of the country members, but it seemed to him that if the resolution was passed the control and appointment of the officers of the Society would be in the hands of touts and wire pullers.

Mr. Common thought that some time for sending in the lists should be fixed, otherwise the scrutineers would not have an opportunity of verifying them if they came late.

The President thought they should be in by 12 o'clock on the day of the meeting.

The resolution, as modified by the President and accepted by Lord Crawford, was then put to the meeting, and carried with six dissentients.

Lord Crawford said, that what he had said before as to the feeling of members of the Society residing at a distance with regard to voting for the election of the Council, applied in every way and in the same manner with respect to voting on all subjects which were brought forward at special or general meetings. He should be against the principle of allowing a general proxy to be appointed who might vote on any subject whatever—for anything—and not for a definite object. For instance, there were many cases in which proxies were legally handed from one person to another, and remained good for 12 months. He should consider that would be highly unadvisable to have in a Society like this; but, where the subject was thoroughly well-known beforehand, and where an opinion would probably be expressed, he saw no objection to it. He concluded by moving:—"That a Fellow of the Society may vote on the business before the annual general meeting, or special general meeting, either personally or by proxy."

Mr. Newall seconded the motion.

Mr. Turner said: That as absent country members would not hear nor understand the various questions which arose without being present, he should be inclined to let them remain as people who had neither voice in the matter nor liability to be influenced.

Mr. Chambers said he was in favour of the resolution as well as of the last.

Mr. Stone said that however much he was in favour of the first resolution, he was equally opposed to the present one. It was extremely undesirable that there should be appeals from the Council to the general meetings. If they had no confidence in the Council, if they were not represented by proper men, let them elect proper men to the Council. There might be disagreements on the Council, and a member would appeal to the general meeting. He did not think anything more deplorable could be conceived than a meeting of this Society being called where no heed whatever was taken of the arguments on the two sides, but the question simply depending upon which member had the largest number of proxies.

Sir Edmund Beckett said he was sorry to say Mr. Stone had not convinced him. Before he spoke he had felt doubtful whether to vote for or against the resolution, but his arguments had convinced him the other way. He did not see why men who were absent had not as good a right to express their opinions on matters of policy as those present, and on the same principle that he voted for the former resolution he should vote for this, after Mr. Stone's speech.

Captain Noble, as a country member, said he should only be too glad to be able to express his opinion without having to come up to London to do it.

Admiral Ommanney asked how many members must be present to constitute a general meeting.

The President replied 30.

Mr. Knobel said the bye-laws did not state the number of members necessary to be at the annual meeting, but it was necessary to have 12 at ordinary meetings.

Dr. Schuster said he felt very strongly on this matter. The only argument brought forward in favour of the resolution was that men were not influenced by discussions. If once that principle was admitted, he thought they should go one step further, and say they would take the voting first and the discussion afterwards.

Mr. Ranyard said if a member chose to vote without attending to what might be said on either side, he certainly had the right to do so. If it was proposed to disfranchise members who had not heard the discussion, they ought to be logical and take away the right of voting from deaf members who attended the meetings, as well as from those who would not listen. The second resolution only proposed that absent Fellows should be allowed to vote on all subjects besides the election of the Council. There were many subjects besides the personal merits of candidates for the Council which could never be properly discussed at the

meetings. The subject of endowment of research could never be discussed, that is to say, nothing against it was ever allowed to be said, for one member after another immediately rose on points of order, though the meeting would listen patiently to pleas for large telescopes, or any other excuse for getting money out of the government. Such matters could never properly be discussed before the Society, and it would be better that the vote should be taken silently.

The resolution was then put to the meeting and lost, 35 voting for and 38 against it.

The President: I have to report officially, that in reply to the post cards sent out by order of a resolution passed at the Annual General Meeting, requesting Fellows to say whether they desired a change in the hour of meeting or not, the following is the result: Those in favour of the meeting continuing at 8 o'clock, 106 ; at 5 o'clock, 125; neutral 150; and other hours, 17; so that it will be seen there is no decided preference, if we take into account the number of neutrals.

Mr. Chambers moved "that in accordance with those figures it is expedient that the hour of the meeting should be 5 o'clock." He complained that Fellows had been asked whether they were "neutral" on the post card, instead of, simply, whether they preferred 8 o'clock or 5 o'clock.

Mr. Ranyard said he thought it was understood at the general meeting, that unless there was a very decided majority in favour of a change, that nothing should be done. There did not seem to be a decided majority.

Mr. Knobel said that if the meeting was to be held at 5 o'clock the Council would have to hurry over a great deal of important business.

Lord Crawford said that had the question been definitely put for 5 o'clock or 8 o'clock, he should have been for 8 o'clock, but it occurred to him that 7 o'clock would have been more convenient, as a great deal of important business was now not done for want of time, and he therefore voted for 7 o'clock.

The resolution proposing that the ordinary meeting be held at 5 o'clock, was then put and lost.

The following papers were taken as read :—

Rev. S. J. Johnson: *Observing weather.*

G. L. Tupman: *Observations of comet,* 1884, *III.* (*Wolf*) *at Harrow.*

N. E. Green: *Observations of Saturn.*

E. J. Spitta: *A note of an observation during the transit of Jupiter's satellite IV., April* 18.

W. G. Thackeray: *On the diameters of the Sun and Moon as observed with the Greenwich transit circle.*

D. Gill: *Reply to Mr. Stone's paper on screw errors, as effecting the N. P. D., of the Cape Catalogue for* 1880.

THE LIVERPOOL ASTRONOMICAL SOCIETY.

The fourth session was closed on Monday evening by a meeting at the Association Hall, the Rev. T. E. Espin, B.A. (President), occupying the chair. In a paper *On the possible origin of comets,* Mr. J. R. Sutton said the recent return of Encke's and Tempel's comets to our evening skies afforded an opportunity of considering what might have been their origin. The periodic time of Encke's comet had been observed to decrease by an average of about two hours in each revolution, and there could be no doubt that this would eventually land it on the sun's surface. The most natural explanation of this peculiarity was that some retarding influence gave the sun's attraction greater power than would otherwise be the case. The theory that comets had been erupted from the interior of planets was very ably argued by Mr. Proctor, but the only factors that could support such an hypothesis seemed to be that the aphelia of periodical comets crowd upon the orbit of Jupiter, and that the ascending node of Tempel's comet was very near to the orbit of Uranus. But the connection which we now know to exist in so many instances between comets and meteor-systems was fatal to Mr. Proctor's view, because, if Tempel's comet were erupted from Uranus, so also must have been the meteor-system to which it belonged. But he contended that no planet could by eruption generate a quantity of matter which would revolve in a continuous ring around the sun. In fact, he would point out that the enormous length of these meteor-systems which form rings, unbroken perhaps throughout their entire extent, quite precluded the idea that they could have originated anywhere within the bounds of the solar system. Whether they came to us from the stars might reasonably form a matter for dispute, though spectroscopic analysis certainly seemed to point to their formation by stellar processes. But certain it was that a sufficient retardation had been proved to render either the planetary capture or eruptive theories unnecessary for the argument.

Mr. W. F. Denning, F.R.A.S., would remind Mr. Sutton that the number of certainly known cases of identity between cometary

and meteoric orbits was remarkably small, amounting, in fact, to only four instances. The connection between comets and meteors was proved rather by the precise agreement of their orbital character than by the mere number of such agreements. He thought, also, it was quite possible that comets might have a derivative source in planetary eruptions. A planet might, for example, have evolved a certain quantity of matter in the form of an immense cloud of meteoric pellets, or, say a volcanic outburst similar to the great Java eruption of August, 1883, and this cloud would, if ejected with sufficient force to leave its parent planet, certainly tend towards the sun, and in process of time assume the form of a meteor-ring. The foremost particles of the original mass would primarily gain ground on the central portion, and this would cause an extension of the stream. Then, as the mass passed perihelion, there would naturally be a slight difference in perihelion distance, and this meant a difference in periodic time. The mass would thus be resolved into a train of particles, and ultimately into one complete ring. Therefore, granting that comets and meteors had been erupted from planets, their subsequent resolution into rings could only be a question of time.

Mr. Joseph Baxendell, F.R.S., &c., said Captain W. Noble had lately drawn the attention of the Royal Astronomical Society to an apparent discrepancy between the proportions of Saturn and his rings as seen in a telescope, and those derived from data given in the *Nautical Almanac.* Since the publication of Captain Noble's communication, it had struck him that a phenomenon he had witnessed in October, 1882, strongly supported the position Captain Noble had taken. At the date mentioned, the outer minor axis of the outer ring had, according to the *Nautical Almanac* elements, become relatively less than the polar diameter of the ball, but he had observed the S. Polar limb of the planet to extend sensibly beyond the southern edge of the outer ring, though no trace of the N. polar limb could be perceived on the northern edge of the ring. This appearance became well marked on the 28th October, when it was readily seen with a power of 180. As the elevation of the sun above the plane of the ring was at this time about 11½ minutes less than that of the earth, the invisibility of the northern limb of the planet could not have arisen from the shadow of the ring being thrown upon it. Assuming the elements in the *Nautical Almanac* to be correct, the only explanation appeared to be that the centre of the ball was not coincident with the centre of the ring-system, but was at a sensible distance on the S. side, and therefore was not the centre of gravity of the planet.

Mr. R. C. Johnson read a paper by Mr. Geo. Knott, LL.B., &c., *On the variable star* U *Geminorum.* It was seldom the case that circumstances favoured the obtaining a complete observation of a maximum of Mr. Hind's remarkable variable star. In addition to cloudy skies, a further disturbing cause was to be found in the moon, whose path was comparatively near the star's place, and whose presence sometimes seriously interfered with, if it did not entirely prevent, all effective observations. The brightness of the star had been observed to vary from 14.5 m. on the 1st of April, 1885, to 9.5 m. on the 6th, when it slightly decreased in brightness. It reached its principal maximum of 9.2 m. on the 8th. It would, therefore, appear that on this occasion the maximum was a double one, with a subsidiary minimum intervening, a phenomenon which had already been observed in the maxima of this star. Professor Schönfeld had pointed out in his *Zweiter Catalog*, that not only were the intervals between successive maxima of this star subject to great irregularity, but that the duration of the maximum phase (or length of time taken in rising to maximum and falling to minimum again) had a varying range of from 10 to 20 days. It would seem, indeed, that the maximum might be broadly divided into two types, one well marked with a duration time of 10 $\pm$ days, the other (not, perhaps, presenting quite such distinct characteristics, but still well marked) with a range of from 15 to 20 days. The longer maximum was sometimes marked by a slight subsidiary minimum. Whether the maximum was of longer or shorter duration the rise appeared to be rapid. He had not found any regularity of sequence as to long or short maxima, nor did there appear to be any decided relation between successive maxima and the lengths of the duration. Neither, on the other hand, did it appear that the point of magnitude touched was in either case higher or lower than in the other. But at the same time it must be admitted that the observations were hardly sufficiently complete to enable one to speak decidedly on points of this nicety.

The President read a paper on the probable period of the variable star LL 14551. This star had been observed to be variable by him in 1883, so photometric observations of it were published in the *Monthly Notices* of the R. A. S., Vol. XLIII. p. 432. During 1884 it had been observed with a photometer of his own contrivance, but only very casually, as the star was badly situated in our English sky. Mr. W. E. Jackson, of Constantinople, had, however, kindly taken up the observations, and, as an example of the magnificent opportunities enjoyed by astronomers in that part of the world, no less than 16 complete observations were obtained last month, 8 of them being on consecutive nights.

From data that had thus been established, a period of 14·09 days was calculated. The photometric observations of 1883, also gave a similar period. The variation in light was very irregular. After a maximum the star decreased for 1½ days, the light then increased for about the same period, and, after remaining stationary for about 2 days, showed again a tendency to increase for 1½ days, after which it diminished, and then remained stationary, rising steadily for 2½ days before the maximum. As this was the first variable star known in this constellation he purposed to call it R. Pippis.

The Secretary read a paper by Mr. J. Baxendell, junr., on transits of Jupiter's IV. satellite. Attention had lately been drawn to the appearance of IV. as a dark spot when transiting the disc of the planet, and it might be worth while to submit a series of his observations of this phenomenon made with a 6-in. refractor. On May 18, 1884, at 9·14 p.m., IV. and also the shadows of I. and II. were on the disc, IV. appeared as a dark spot, almost like a shadow, and nearly if not quite as dark as the shadow of I. The shadow of two was not so dark, but more like Indian ink, and not more than one-third of the diameter of the shadow of I. On February 27 last, at 10·15 p.m., IV. appeared as a very black spot, much darker and better defined than the shadow of I. On 18 April at 7·50 p.m., IV. was observed not far from the *f* limb, but not nearly so black as on previous occasions, though two hours later it was seen to be much darker, whilst at 11·15, when it was very near the *p* limb, it was scarcely to be distinguished from the planet. Altogether, the observation seemed to confirm the statement lately made by Mr. Knobel, that IV. appeared much darker in the middle of its transit than when near either limb of the planet.

In a paper on the formation of lunar bays, Mr. S. E. Peal called attention to the shapes of Le Monnier and Fracastorius, which he thought seemed to show that they were the remains of circles once complete, and now probably deformed by some influence of the adjoining mare, which had bodily removed the plain from that side. Those who held that the bays were ruins of complete circles, were however met by two insoluble difficulties; first, as to how such a huge circle could have been found, second how so large a segment could have been so completely removed as to leave not a trace behind, and which, on the volcanic hypothesis appeared to be impossible.

SELENOGRAPHICAL NOTES, JUNE, 1885.

By Thos. Gwyn Elger, F.R.A.S.

Piazzi Smyth, Kirch, and the neighbourhood.—That portion of the Mare Imbrium which includes these objects and extends between them and Plato is of especial interest to observers desirous of studying the configuration and character of lunar ridges. Running in N.N.E. direction from Piazzi Smyth towards the western border of Plato is one of the broadest and most noteworthy examples to be found on the moon's surface. Under morning illumination, and when the terminator passes near the brilliant Pico, the very intricate details of this remarkable object are observed to the best advantage. It is then seen to be made up of a number of long, narrow, interlacing minor ridges, with deep intervening depressions, and is devoid of that roundness of outline characteristic of the generality of these features in other regions. As it proceeds northwards it increases in height and breadth, and throws out three branches towards the east, the two nearest Plato forming the southern boundary of the partially enclosed area, termed by Schroeter, Newton, and the other running towards the triple-peaked mountain B (*Neison*, Map VI.) south of Pico. The border of the bright little ring-plain, Piazzi Smyth, is not continuous. There is a very distinct break in the north wall a little west of the place where the great ridge joins it. The shadow of this formation at sunrise indicates the presence of a considerable peak on the south-eastern rim. South of the ring-plain is a remarkable curved ridge, with its convexity turned towards the east, which ultimately extends to the mountain mass T (*Neison, Map IX.*), and throws out a branch to Kirch. Another very similar but shorter ridge also originates at Piazzi Smyth and runs southwards, the entire system very much resembling that associated with Burg. The peculiar configuration of the ridges in this neighbourhood impresses one with the idea that they are of more recent date than Piazzi Smyth, inasmuch as both north and south of this formation, but especially on the south, its presence seems to have occasioned a deflection in the material, whatever it may be, of the sea, such as we may suppose would be caused by the interposition of an obstacle to the progress of a semi-liquid mass flowing towards the east, which is now manifested by a heaping-up of this material in the form of banks and ridges. A very similar appearance may be noted south of the neighbouring mountain, Piton, where a ridge is also deflected towards the east. There are many unrecorded craterlets, large and small, surrounding Piazzi Smyth, and lying between it

and Kirch. One of the largest, nearly midway between these formations and due east of a small but lofty isolated mountain, is very conspicuous. A faint curved light streak extends for some distance to the north-east of Piton. It does not appear to cast a shadow, and is possibly a cleft.

Menelaus. While observing the southern border of the Mare Serenitatis between 8h. and 9h. on 21st April, two large oval-shaped elevations were remarked on the eastern half of the floor of Menelaus, the remaining portion of the interior, including the central mountain, being in shadow. The brilliant quadrangular formation b, south-west of Menelaus, appears to consist of four distinct ridges inclosing an area much darker than its surroundings, and including two isolated hills. Schroeter (T. XI.) shows this object as a large crater, *a*, and inserts a somewhat smaller crater, *b*, on the north of it, of which there is now no trace beyond an ill-defined white spot, which I have never seen as a depression. The region immediately on the south-east of Menelaus is remarkable for its dusky tone, and for two very curious large square depressions, of great depth, associated with smaller depressions of a wedge-shape. The border of the Mare between Menelaus and Sulpicius Gallus should be carefully searched for the probable westerly extension of the clefts found in the neighbourhood of the latter formation.

Kempston, Beds: May 19, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

SEARCH FOR VULCAN.

Again at the spring node of the accepted orbit of the suppositious Vulcan, the sun was examined here each day (except Sundays) when the weather allowed, over the period during which a transit is possible, according to Leverrier, but with no positive result. It may be stated with confidence that no planet-like object, certainly none with motion relative to spots, &c., was visible on the sun's disc at the times mentioned below with no modifying note; the figures giving the middle times of

the length of each scrutiny. The letter *b* signifies that the observation was by myself, all the others being by assistant:

Mar.	h.	m.	Mar.	h.	m.	April.	h.	m.
15	23	50	27	22	38	7	1	24
16	3	20	28	0	57		4	8 (*b*)
	22	20		4	31		20	13 (*b*)
17	3	15	30	4	19 (*b*)		23	26
18	1	29 (*b*)		5	37 (*b*)	8	4	6
	4	10		6	15‡ (*b*)	12	19	58 (*b*)
	22	27		19	59 (*b*)		22	27
19	0	40 (*b*)		22	18	13	0	40 (*b*)
	1	17	31	0	29*		21	28
	22	35	April.				23	49
	1	8 (*b*)	1	19	57 (*b*)	14	1	37 (*b*)
	4	38		22	15		3	38
	20	31 (*b*)	2	0	54		6	9
	21	50		3	28		21	36
21	0	15		5	27		22	52 (*b*)
	3	0		19	51 (*b*)		23	31
	4	50		22	23	15	3	36
22	21	45	3	1	45*		5	55
	23	50		4	7		23	25*
23	1	25		5	56	16	1	35
	3	10*		20	4 (*b*)		6	0
26	3	54* (*b*)		22	54		20	31
	5	20‡ (*b*)	4	0	52		23	10
	23	15*		3	44	17	1	36
	23	45†		6	5		5	18
27	1	40	5	20	13 (*b*)		21	28
	3	16 (*b*)		21	52	18	21	22
	5	15	6	0	27†			
	20	5 (*b*)		23	13			

* Observation not good.
† " " very good.
‡ Definition bad.

In a previous communication on this subject, Vol. XXII., p. 273, the note "imperfect observation" was omitted for that on September 25.

I consider the question of the existence of Vulcan can only be settled by continued and *systematic* watching for its appearance in transit, as at the time of an eclipse of the sun it may be too near that luminary to be visible.

T. W. BACKHOUSE.

Sunderland: May 19, 1885.

THE RED SPOT ON JUPITER.

This spot is becoming more conspicuous, and I cannot see any difference between its present appearance and that when it was most conspicuous, except that it is fainter now. Whether this is owing to any real change since Mr. Denning's observations, or to the small power of my telescope of 4½-inches aperture, those with larger instruments will be able to decide.

Sunderland: May 19, 1885. T. W. BACKHOUSE.

SUNSET ON PLATO.

SIR,—The phenomena attending sunrise upon the lunar walled plain Plato have been ably considered by Birt in the British Association reports for 1871, 1872, and are of extreme importance and interest. Of not less importance, likewise, must be considered the various circumstances connected with sunset upon the same magnificent formation. As I do not think any account of these have yet been published—at least, of the actual disappearance of sunlight from the floor—and the whole phenomenon is one of unusual grandeur and impressiveness, it may not be out of place to give a short description of the progress of sunset.

The following account is gathered from a long and connected series of observations which have been made of Plato by the Rev. F. B. Allison, at Chesterfield, Mr. T. P. Gray, at Bedford, and the writer at Brighton, since the year 1879. Schroeter remarks of sunrise, that one may watch for a year and yet not see it, so short is its duration, and even rarer appear to be the opportunities for observing sunset, for during a period of six years, in which over 400 separate observations of Plato have been obtained, only three complete views of sunset have been secured.

About three days after the full moon, the first signs of the approaching event are afforded by a slight line of shadow under the eastern wall, and a brightening of the eastern portion of the floor; but it is not until several days later that the appearances portending the approach of night become at all conspicuous or striking. The shade cast by the east wall is then considerably increased, and a little north of the centre of the floor a remarkable spire of shadow is visible projecting on the floor far in advance of the general line of shade, and becoming increasingly conspicuous with the advance of time. This is the shadow cast by the mountain mass on the north-east wall marked ζ by Beer and Mädler, and sometimes known by the name of Rupes Smythii, a lofty mountain more than 8,000 feet above the level of the interior.

When the moon is nearly 22 days old, this spire of shadow stretches across the floor nearly to the west wall, its southern edge being usually about in contact with the north edge of the small central crater, though there are slight variations, according to the position of the sun. Two short and comparatively insignificant peaks of shadow are then also to be seen to the south of the shade cast by ζ, and one to the north. At this time few of the light spots and craterlets are visible. The central crater, No. 1, is generally apparent, with sometimes 4 and 17, whilst 22, close to the west wall, is at such times more conspicuous than the others. Spot No. 42, close under the south-west wall, and No. 48, a little to the north of 22, have also been seen shortly before sunset, but the great obscurity of the floor at this time renders it difficult to pick up delicate features. The shadow projected by Rupes Smythii presently reaches the foot of the west wall, gleaming brilliantly under the rays of the declining sun, and it may be known by this, that the time when the sun will finally depart from the floor of the great inclosed plain is not far off, the eastern half being already nearly hidden in the shadow. The appearance now rapidly changes, and in the course of a little over an hour the interior of Plato becomes quite filled with shadow, with the exception of a faint ill-defined patch near the middle and a little to the south of the centre of Plato, and another smaller and fainter one on the north-west part of the floor, where the sunlight still faintly lingers. Ten minutes or a quarter of an hour longer is now sufficient to see the disappearance of these last vestiges of the departing day; first the smaller patch disappears from view, and lastly, a few minutes later, the larger one gradually fades away, so gradually that it is impracticable to determine exactly when it does disappear, and for some time after one occasionally fancies a slight glow still prevails at this part.

In the meantime, it is an interesting sight to watch the extremity of the shadow cast by the mountain ζ perceptibly ascending the brilliantly illuminated eastern slope of the west wall of Plato. About 18^m after the last trace of sunlight has disappeared from the floor, the summit of the wall is reached, and as there is now shadow extending far to the westward, the very bright west wall is completely divided into two by the shadow of this mountain, more than 60 miles distant. An observer seeing Plato at this time, with the west wall thus cut in two, and unacquainted with the progress of the phenomenon, might think that there was a valley or depression at this part of the wall, and consider it confirmatory of the well-known observation of sunrise by Bianchini on August 16, 1725, when a ruddy track of light across the middle of the floor was thought might

indicate the existence of a gap in the wall at this very same point, but in this he would not be correct.

It is desirable that as many of these important occurrences as possible should be watched, and whenever sunset is seen to be approaching, every effort should be exerted to take advantage of the rare opportunity. Whenever the spire of shadow projected by the mountain ζ is seen to extend nearly to the foot of the west wall, it may be known that sunset will occur in the course of a few hours. Any trifling inconvenience will be well repaid by witnessing one of the grandest and most impressive of lunar spectacles.

Yours faithfully,
A. STANLEY WILLIAMS.

West Brighton: May 19, 1885.

THE REV. PREBENDARY WEBB.

We regret to announce the death of the Rev. Prebendary Webb, which took place early on the morning of Tuesday, the 19th May, at Hardwick, near Hay, on the Welsh border. He was the only son of the Rev. John Webb, an antiquarian of some note, who wrote "The History of Herefordshire during the Civil War," and edited the "Memoirs of Colonel Birch, some time Governor of Hereford during the Civil War," both of which works were completed and edited, with valuable notes, by his son, the Rev. T. W. Webb, after his father's death.

Thomas William Webb was a Fellow Commoner at Magdalen Hall, Oxford, and took a second class in mathematical honours in 1829. He was ordained a curate on August 1st of the same year in Hereford Cathedral, and licensed to the curacy of Pencoyd. In 1831 he was ordained priest, and became minor canon and precentor in Gloucester Cathedral, after which he took the curacy of Tretire with Michael Church, where his father was vicar. He worked under his father till 1856, when he was given the living of Hardwick, a large, though thinly populated mountainous parish, to which he devoted himself for nearly 30 years, always giving the greater part of the day to parish work. Here he wrote "Celestial Objects for Common Telescopes," a book to which thousands of amateur astronomers are indebted, and of which it may, without exaggeration, be said that it has done more to interest observers in the heavens than any other book that has been published. Most of the objects described were examined by Mr. Webb with a 9-inch silver on glass with reflector, mounted in a small wooden observatory in the rectory garden.

He also published a popular book on the sun, and a book on optics for younger students, entitled, "Optics without Mathematics," besides a very large number of articles on Observational Astronomy, published in "The British," "Fraser's," and "The People's Magazine," "The London Review," "The Intellectual Observer," "The Student," "The Popular Science Review," "The Argonaut," "The English Mechanic," "Nature," and "Knowledge." In 1882 he was appointed one of the prebendaries of Hereford Cathedral, an office which required only one month's attendance at the cathedral during the year, and would have separated him but little from the parish to which he was devoted. If he had lived a few weeks longer he would have completed his 80th year.

A. C. R.

REVIEWS.

The *Bulletin Astronomique* of the Paris Observatory for February, contains among other articles, certain particular solutions of the problem of three bodies, by M. H. Poincaré; notice on the telluric rays of the solar spectrum, and on the group α in particular. A new practical method of distinguishing by simple inspection, rays of terrestrial from those of solar origin, has led to the establishment of the intimate relation of the group α with the bands A and B of Fraunhofer, and of its being due to absorption by the oxygen of the air. This was the opinion of Angström and Egeroff, and it was suspected by Piazzi Smyth (Madeira Spectroscopic). "There yet remain, as may be supposed, many points to be determined for the complete knowledge of all the telluric groups of the solar spectrum, but instruments and methods of observing are being perfected, and we may hope that a not distant time will give us the solution of these problems, of so high interest, both as to terrestrial and astronomical physics." The present number of the *Bulletin* contains copious reviews of astronomical publications, and the Séance of the Council of the Observatory of Paris, January 30th, 1884. The revision of the catalogue of Lalande will soon be finished, and the printing has begun. In 1883, 23,830 meridian observations were made. For the equatorial of the E. Tower, an excellent objective of 15 in., has been constructed by M. M. Henry, with which the second satellite of Mars was observed in January. This is the best and most powerful glass which the Observatory of Paris ever had. The magnetic observatory was installed in the subterranean halls at the end of last year, and observations are regularly made. During the year the volume of the observations of 1880, and the XVIIth volume of the *Mémoires* have been printed. Increasing inconveniences make it more than ever desirable to carry on the principal work of the observatory in a branch established outside of Paris. M. Mouchez proposes to meet the cost of this by the sale of a portion of the gardens round the observatory, retaining its actual buildings and the ground indispensable for observations, to which the establishment would continue to be devoted. It is to be hoped that this most essential change, one desired by all astronomers, and sanctioned by the Academy as far back as 1869, may speedily be effected.

ASTRONOMICAL OCCURRENCES FOR JUNE, 1885.

DATE.		h. m.	Principal Occurrences.	Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m Antares
Mon	1		Sidereal Time at Mean Noon 4h. 40m. 32·00s.			11 40·1
Tues	2		Sun's Meridian Passage 2m. 16·24s. before Mean Noon	1st Tr. I.	10 50	11 36·1
Wed	3					11 32·2
Thur	4			2nd Ec. R.	11 14 9	11 28·2
Fri	5	12 4 7	☾ Moon's Last Quarter Conjunction of Neptune and Mercury 1° 6′ S.	1st Oc. D.	10 43	11 24·3
Sat	6			1st Sh. I. 1st Tr. E. 1st Sh. E.	9 16 10 22 11 35	11 20·4
Sun	7	10	Conjunction of Saturn and Venus 1° 32′ N.	1st Ec. R. 3rd Sh. I. 4th Tr. I.	8 42 3 9 2 12 2	11 16·4
Mon	8					11 12·5
Tues	9					11 8·6
Wed	10	11 13	Conjunction of Neptune and Mars 1° 29′ N. Conjunction of Moon and Mars 3° 51′ N.			11 4·6
Thur	11	4	Conjunction of Moon and Mercury 2° 47′ N.	2nd Oc. D.	8 36	11 0·7
Fri	12	10 42 18	● New Moon Conjunction of Moon and Saturn 4° 3′ N.			10 56·8
Sat	13	5	Conjunction of Moon and Venus 5° 48′ N.	1st Tr. I. 1st Sh. I.	10 0 11 10	10 52·8
Sun	14		Sidereal Time at Mean Noon 5h. 31m. 47·24s.	3rd Tr. I. 1st Ec. R.	8 15 10 37 11	10 48·9
Mon	15		Illuminated portion of disc of Venus=0·981 Illuminated portion of disc of Mars=0·975			10 45·0
Tues	16		Sun's Meridian Passage 0m. 25·60s. after Mean Noon	4th Ec. D.	10 24 57	10 41·1

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Antares
Wed	17	3	Conjunction of Moon and Jupiter 3° 44′ N. Saturn's Ring: Major axis=37″·35 Minor axis=16″·84			10 37·1
Thur	18	11	Conjunction of Saturn and Sun	2nd Oc. D.	11 17	10 33·2
Fri	19	1 48 14	☽ Moon's First Quarter Uranus at quadrature with the Sun			10 29·3
Sat	20			2nd Tr. E. 2nd Sh. E.	8 33 10 46	10 25·3
Sun	21			1st Oc. D.	9 9	10 21·4
Mon	22			1st Tr. E. 1st Sh. E.	8 47 9 53	10 17·5
Tues	23	16	Conjunction of Saturn and Mercury 1° 41′ N.			10 13·5
Wed	24					10 9·6
Thur	25			3rd Ec. R.	10 30 7	10 5·7
Fri	26	23 17	○ Full Moon			10 1·7
Sat	27	3	Superior conjunction of Mercury and Sun	2nd Tr. I. 2nd Sh. I.	8 23 10 28	9 57·8
Sun	28					9 53·9
Mon	29	12 13 7 13 36 14 22	Occultation of 13 Capricorni (6) Reappearance of ditto Occultation of 14 Capricorni (5) Reappearance of ditto	1st Tr. I. 1st Sh. I. 1st Tr. E.	8 26 9 28 10 46	9 49·9
Tues	30			1st Ec. R.	8 56 8	9 46·0
JULY Wed	1	14 37 15 59	Occultation of B.A.C. 7774 (6) Reappearance of ditto			9 42·1

THE PLANETS FOR JUNE.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	3 8 53	N.14 38¾	7″·0	
	9th	3 57 50	N.18 43½	6″·0	
	17th	5 1 3	N.22 31	5″·4	
	25th	6 15 46	N.24 30	5″·0	
Venus ...	1st	5 10 8	N.23 17¼	9″·7	
	9th	5 52 58	N.24 4½	9″·8	
	17th	6 36 2	N.24 7	9″·8	
	25th	7 18 50	N.23 23	10″·0	
Jupiter ...	1st	10 3 13	N.13 7	33″·3	
	9th	10 6 54	N.12 46	32″·5	
	17th	10 11 4	N.12 22	31″·9	
Uranus ...	2nd	11 56 47	N. 1 9½	3″·8	7 11·1
	18th	11 57 1	N. 1 7½	3″·8	6 8·4

Mercury rises on the 1st, three quarters-of-an-hour before the sun, the interval slightly increasing till the middle of the month, when it rapidly decreases.

Venus sets 40 minutes after the sun at the beginning of the year, the interval increasing.

Jupiter sets about half-an-hour after midnight on the 1st, and then earlier each night.

Books received :—Observatory.—Directors' Annual Astronomical Report of Harvard College. By Ed. C. Pickering.—Observations of Variable Stars in 1884. By Ed. C. Pickering.—Illustrated Science, Monthly: a Catalogue of Suspected Variable Stars. By J. E. Gore, M.R.I.A.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1884.	To Dec., 1885.
Mrs. Jackson Quilt.	W. Thornthwaite.

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The Astronomical Register.

No. 271. JULY. 1885.

ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

The fourth meeting of the session was held at the Society's apartments, Burlington House, on Friday, June the 12th, 1885.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut.-Col. Tupman.

The Rev. Thos. Perkins, M.A., of Shaftesbury School, Shaftesbury, Dorset,

was balloted for, and duly elected a Fellow of the Society.

Colonel Tupman announced that 62 presents had been received since the last meeting; among them is a photograph of part of the milky way, taken by the brothers Henry of Paris, with a new object-glass of their own construction, of 13-in. aperture, and $11\frac{1}{2}$-ft. focal length, mounted side by side on the same axes with the great refractor. The photograph was exposed for 60 minutes, and has been enlarged twice. It shows stars of the 14th magnitude.

Mr. Knobel read a paper by Mr. W. F. Denning *On the Velocities of Meteors.* The author referred to the papers read at the Society in December and May, and the discussion upon them as to the velocities of meteors and the speed necessary to explain the occurrence of fixed and long-enduring radiant points. He desired to say that no such velocities had been observed, and that it was impossible that the fixed radiant points could be explained on the supposition of such great velocities. He agreed with Colonel Tupman as to the difficulty of accounting for these long-enduring showers, and that the velocities referred to by

Mr. Proctor had no existence. The meteors coming from the stationary radiants appeared to have velocities which did not differ materially from the parabolic velocities exhibited by planetary meteors. Mr. Ranyard had asked whether the speed of meteors from any of the stationary radiants had been determined. There were several instances in which such determinations had been made, and the velocity had been found not to differ greatly from that of the planetary meteors, allowing for a wide limit of error; but it was impossible that the velocity could be anything approaching that suggested as the only explanation of the long-enduring showers. The paper concluded by giving some instances to show that the observed motions gradually decreased as the radiant recedes from the earth's apex, and that was probably the true explanation of some of the fixed radiant meteors appearing to have velocities greater than that of the planetary streams.

Mr. Ranyard: This seems to me a very important series of facts that Mr. Denning has communicated. The paper appears to show that the velocity of meteors from the fixed radiants is not very great as compared with the parabolic velocity. If this is really the case I think we may take it as certain that these meteors do not come from outer space beyond the planetary system; for, if the velocity of a meteor shower coming from outer space were at all comparable with the velocity of the earth in its orbit, the radiant would, in the course of the year, move in a circle many degrees in diameter, because the direction in which meteors appear to approach the earth is not the direction in which they are really moving in space; but it is determined by the relative motion, which is compounded of the meteor's motion and the earth's motion in its orbit. Meteors moving with a velocity fifty times as great as the ordinary planetary meteors, would probably give rise to quite a distinct class of phenomena. The heat generated is proportional to the square of the velocity, and consequently at the same altitude such rapid-moving meteors would be consumed in an incredibly short space of time. Planetary meteors become incandescent at a height of 70 or 80 miles above the earth's surface; and though it is possible to conceive that meteors moving with a velocity fifty times as great might become incandescent at 20 or even 50 times as high in a region where the atmosphere is very much rarer, with such enormous velocities it seems probable that the process of burning the meteor would be much more rapid, and that the air would behave almost like a solid, and the meteor would be broken up with a rapidity almost comparable to the duration of an electric flash.

Colonel Tupman: I do not think the Fellows of the Society quite grasp Mr. Ranyard's and Mr. Proctor's ideas on this subject. I will endeavour to explain. Suppose this to be the earth (illustrating on the black board), and this the limitation of the earth's atmosphere, say about 100 miles high. Shooting stars at that height, or rather at 50 or 60 miles, have been measured in several thousand instances from radiants in all parts of the sky. The height at which they begin to appear may be said to be constant for all of them. The relative velocity of meteors which meet the earth is greater than the average. and they begin to appear a little higher, like the November meteors, for instance. In regard to those meteors which overtake the earth, their relative velocity may be reduced to 7 or 8 miles a second; and they do not appear so high, but there is little difference between the extremes of the heights which have been measured. The only way in which Mr. Proctor's assumption of increased velocities could possibly exist would be if the meteors encountered the atmosphere at much greater heights, say at 500 or 600 miles, so as to preserve the average apparent angular velocity; and that might have held good, although improbable, if it had not been for the fact that the height of individual meteors has been actually measured, over and over again, from these radiants, and we are therefore driven to the conclusion that these long-enduring radiant meteors appear at no greater elevation than the others, and therefore that their velocities are no greater than the velocities observed in all other kinds of meteors, viz., that which is called parabolic.

Colonel Tupman proceeded to explain, by means of a diagram, the relation between parabolic velocity and the velocity of the earth in its orbit.

Mr. Bryant read a paper *On the Elliptic Elements of Comet (Ross) II.,* 1883. The orbits, of which elements have already been published, of this comet, do not present such an agreement as might have been expected on the assumption that the comet is moving approximately in a parabola. An ephemeris being computed from approximate elements, comparison was made with every published observation of the comet. By the advice of Mr. Hind, he had rejected all observations except those of Mr. Tebbutt; but he still failed to find a parabolic orbit which would correspond to them. Mr. Bryant, therefore, undertook to investigate its elliptic elements, and, by means of differential equations, arrived at two sets of elements, both of which satisfied Mr. Tebbutt's observations with a greater error of six hundredths of a second of time in right ascension. One gives a period of 87 years, and the other a period of 94 years.

Mr. Knobel: As we are rather short of papers, I may be permitted to bring forward some questions with regard to the historical part of astronomy. Mr. Knobel then read a paper *On Al-Sufi's star magnitudes.* He said that in the 14th vol. of the "Annals of Harvard College Observatory," Professor Pickering had called attention to some alleged differences in the star magnitudes of Al-Sufi and Ulugh Beg, which might lead one to infer that Ulugh Beg had made some original observations of the brightness of stars. In the *Monthly Notices* for 1879, Mr. Knobel had given a complete translation of the magnitudes in the R.A.S. Persian MS. of Ulugh Beg, a reference to which would have disposed of a large proportion of the alleged differences. To investigate the point, he had translated the magnitudes of Al-Sufi, as given in two Arabic MSS. in the British Museum and the India Office libraries, and appended to the paper the different readings he had found, compared with Professor Schjellerup's translation of MSS. at St. Petersburg and Copenhagen. The result arrived at was to show that it is extremely doubtful whether any difference exists between Ulugh Beg and Al-Sufi, except what is due to errors of transcriber or translator.

The President: We are very much indebted to Mr. Knobel for the way in which he goes into these historical subjects. I think no other person now amongst us would have the patience to investigate such ancient MSS. I have no doubt that many present are interested in what Mr. Knobel has explained to us in his interesting paper.

Mr. Knobel read a paper by Professor Pritchard entitled, *An Inquiry as to the grounds for Criticism by the Astronomers of Harvard College on the Oxford Stellar Photometry.*

Professor Pickering had stated that the results obtained with the wedge photometer, should theoretically be affected by the brightness of the background of sky. This statement surprised Professor Pritchard, because in the application of the wedge he had taken the most scrupulous care, lest any such systematic error should lurk in. He had especially felt it to be necessary to be practically assured that even bright moonlight produces no sensible effect on the determination of magnitudes. In order to test Professor Pickering's assertion, Professor Pritchard had examined about 1,000 observations of Polaris which had been made at Oxford during the last three years; he had had the observations divided into four classes, corresponding with the four phases of the moon, but the part of the wedge at which the star was extinguished, was found to be practically the same, whether the moon was full or new, there being only a difference

of about one-hundredth of an inch in the average reading of the wedge for full moon and new moon, which would correspond to a difference of about one-fiftieth of a magnitude. The four readings were 4·77, 4·76, 4·74, and 4·75 inches on the scale of the wedge.

Professor Pritchard therefore claimed on behalf of the wedge photometer that no systematic error existed in it, either in relation to the varying brightness of the field of view, or to the brightness of the stars themselves. It was important, however, to see what reliance could be placed on the accuracy of the investigations made at Oxford and Harvard. The accuracy of the magnitudes determined at Harvard depended on the separate determinations, made on three different nights. Great exactness could not be necessarily conceded when the observations were limited to three nights. These results could be compared very soon with the Oxford work, because that work would shortly be published by the University. Whatever the results of the comparison were, one thing would be certain, viz.: that the application of photometry to the determination of stellar brightness would be found to have made an enormous advance, since, by the application of the wedge, the difference in magnitude of any two stars might be ascertained with a limit of error not exceeding one-tenth of a magnitude. He hoped that no remark he had made would be found inconsistent with the respect due to the remarkable ingenuity and labour which characterized every page of the Harvard College Observatory work.

Mr. Stone: As far as I can understand Professor Pickering's work it seems that from his experience he thought that three determinations of the magnitude of a star would be sufficient to fix the magnitude within the degree of accuracy he hoped for. But as in the case of cloud the measurement might be disturbed, he has multiplied the observations and taken the mean. In a work of this kind a certain number of discrepancies must be expected; and Professor Pickering has shown, by multiplying his observations, that he did know these discrepancies would occur. It therefore does not, in my opinion, give anything like a fair idea of the accuracy of Professor Pickering's work to pick out and compare the extreme measures where the number of observations has been enlarged. As far as I have been able to examine the work it gives evidence of extreme accuracy with regard to the point that Professor Pritchard mentions: that he has not been able to detect any difference of magnitude of stars, measured during bright moonlight, or no moonlight at all. I must confess that it appears to me to be a proof that the photometer is not accurate enough to discriminate the differences. I

do not know the extent to which the observations have been made purely differentially; but if the result be to prove that the absolute magnitude of stars is the same under different atmospheric conditions and illumination, I should be afraid that it must be taken to show that the photometer is not as accurate an instrument as has been supposed.

The Astronomer Royal: I have had the advantage of examining Professor Pritchard's work, and I must say I think the Society ought to congratulate him upon the accuracy he has introduced into celestial photometry by his wedge photometer. I was somewhat prejudiced against this instrument, so I think my testimony in favour of it may be accepted all the more readily. I had always thought that the point at which a star would be apparently extinguished would vary very much with the sensitiveness of the eye at the moment. I found this was not the case with Professor Pritchard's measures. You have seen by the results given to-night that the point of extinction, which is an absolute measure, and not a relative measure, varies simply by $\frac{3}{50}$ths of a magnitude for Polaris under the different circumstances of bright moonlight and no moonlight. I think that no photometric results yet given are anything like so accordant. One naturally feels suspicious when results are too accordant. Thinking that the observations may have been repeated in the same way so as all to be subject to similar errors, I have looked into these points, and examined the results as far as I could; and I must say, so far as I have gone, I have satisfied myself that there is nothing at all suspicious about them. This method of extinction seems to me a valuable method in photometry. It comes to this, that the sensitiveness of the eye being practically constant, instead of comparing one quantity with another, and combining the errors of two measurements, we have only one measurement to make, and only one error to deal with. That is an important point. There is another question raised by Professor Pickering, which Professor Pritchard has met by bringing forward the numerical result—namely, that of the influence of moonlight; but, besides the mere numerical result there is a common sense point of view of the question. Suppose we have a star seen on a bright background of moonlight, and we apply the wedge photometer, which, by extinguishing the light, makes the star disappear. It is admitted that the star's light is enormously brighter than the background. It stands to reason that long before the star has disappeared by diminishing its brightness, the background will have faded out of view, and will have no effect on the retina. It is well known that light below a certain intensity has no effect upon the retina. When it

becomes so faint as to be insensible it is the same as absolute blackness. In that case, when we apply the wedge, the star itself, which is perhaps 100 or 1,000 times brighter than the background of the sky we are looking at, is just on the point of disappearance. Thus, the background of the sky illuminated by the moon is practically absolute blackness as far as the eye is concerned. Therefore, I think this objection falls utterly to the ground. I can conceive it possible that, as suggested by Professor Pritchard, there might be some residual effect from the background if we compare two stars of different brightness (as in Professor Pickering's photometer), because we have the scattered light from one star, which may be brighter than the other, interfering to a certain extent with the light of the fainter star. It seems to me that the wedge photometer is a very simple instrument, in which there are very few sources of error, and the only source of error that I see is, that the sensitiveness of the eye may vary from time to time and from instant to instant. The simple test of that is, whether we get the same reading for a star of constant brightness, like Polaris, from night to night. Professor Pritchard has given the actual readings at which Polaris disappeared; and if these agree within a small fraction of magnitude, it is to be presumed that his measures give not only the relative magnitude, but that they give practically absolute measures of brightness.

Mr. Stone: Are all the observations given, or only a selection?

The Astronomer Royal: I do not know whether Professor Pritchard has published them all, but I looked into them myself critically. I am not competent to stand forward and propound Professor Pritchard's methods. I can only give them second-hand. I was very much interested in the question, and disposed to doubt his results. I felt they were almost too good to be true. My own experience of measuring star magnitudes did not lead me to believe they could be measured so accurately. Professor Pritchard showed me the results obtained with two different telescopes, and with different observers, and with a different apparatus, and he also showed me the different individual readings. I cannot say whether they are printed, but I had the opportunity of examining them just as they were, and I could form a general idea of their accordance, and was very much impressed by that.

Mr. Common: I am extremely surprised that the extreme range of difference of magnitude is so small as $\frac{3}{50}$ths.

The Astronomer Royal: That is the mean of a large number of observations.

Mr. Common: What is the extreme range?

The Astronomer Royal: I may explain that when I spoke of $\frac{3}{10}$ths of a magnitude it was in reference to the possible effect of the moonlight, and in each case there were about 1,000 observations; I believe that is a question of the effect of moonlight, and is quite a different matter. I do not mean to say if you were to take three observations that you would expect any three to agree within $\frac{3}{10}$ths of a magnitude exactly. There are two different questions involved: one is whether there are systematic errors depending upon moonlight, and the other is whether there are large accidental errors of observation. Professor Pritchard's point is that in his observations there is no appreciable systematic error depending on moonlight, which he seeks to prove by bringing forward his observations of Polaris grouped according to the quarters of the moon in which they were made; and the other is as to whether there is a large accidental error either in his observations or in Professor Pickering's, and he implies, I think, that there is a much larger error in Professor Pickering's observations, which involve the measure of two quantities, than in his own, which only involve one extinction—*i.e.*, the measure of one quantity.

Mr. Common: Professor Pritchard, in his paper, as I understand, gives certain points of extinction, and the number that represents the extent of the wedge in or out for extinction, which I suppose is the average of a number of observations.

The Astronomer Royal: There is only one observation.

Mr. Common: He does not state what is the extreme range of difference between the separate measures; and surely, if they are an absolute measure, there must be nights when you find, according to the telescope you use, there are one or two magnitudes of stars more or less visible. There must be a range to this extent. It appears to me that Professor Pritchard gives the average of his readings, and he gives the extremes of Professor Pickering's, and not the average.

Mr. Common: But he gives the ranges.

The Astronomer-Royal: I wish to try and elucidate the question as explained to me by Prof. Pritchard. Prof. Pickering has raised this objection to Prof. Pritchard's results, that they are affected by the state of the moon. In order to show that there is no appreciable effect from the moonlight, he examines his observations of Polaris in groups arranged according to the first quarter, second quarter, third quarter, and fourth quarter of the moon, so as to get the varying effects of moonlight. That is merely to show whether there is an effect caused by the moonlight or not. Another question is to explain a discrepancy between Prof. Pritchard's results and Prof. Pickering's results,

and he examines the individual observations made by Prof. Pickering to show they are liable to considerable accidental errors. There are systematic errors which might conceivably affect Prof. Pickering's results. There are also accidental errors which Prof. Pritchard conceives affect Prof. Pickering's results, making them less trustworthy when the number of observations is confined to three. He does not dispute that if Prof. Pickering had made a large number of observations of one star, he could get an accurate result; but he considered his own results, with a limited number of observations, are much more accurate than Prof. Pickering's; and so far as that view goes I should be inclined to go with him.

Mr. Common: I agree with him, that, given a certain purity of sky, the moon would have no effect at all, or very little, if the star's light is much greater than the dispersed moonlight. If you extinguish that light before you extinguish the star it would have no effect at all. It is not apropos to the subject, but I may mention now, that I do not believe either in Prof. Pickering's photometer, or in Prof. Pritchard's. The only photometer we shall ever have to be of any use whatever is photography. (Laughter.) There is no personal equation there. I will say what I have seen lately. In 1882 and 1883 I took some photographs with a Ross's lens over a certain portion of the heavens, and I put them away at the time without further attention; but lately I have had the chance of comparing them with photographs taken at the Cape of Good Hope by, I believe, Mr. Woods, and the difference in certain parts of the heavens is enormous; and from the investigation of many photographs of the nebula and stars I am led to believe that every star in the heavens is more or less subject to variation in light, and that a great deal of the difference in these photometric estimates is due not so much to the errors of observers as to the difference in light given by the star. I believe the only way we can get to any absolute determination of the light of stars is by photography, and I think that will be the photometer of the future. It will, of course, only give their chemical power.

Mr. Stone: With regard to this method of extinction, I had, while at Greenwich, some experience of it. I had a small photometer of my own consisting of two tourmalines, and by turning one relatively to the other, I could get the angle of extinction. I found I could make very good differential observations; indeed I found also, by taking another person (and the person I took was Mr. James Carpenter) that his differential results agreed exceedingly well with mine, but the angle of extinction which Mr. Carpenter gave differed entirely from the angle of extinction which I gave.

The Astronomer Royal: Was it constant for yourself and Mr. Carpenter on different nights? that is the point here.

Mr. Stone: All I can say is, by using this instrument, and taking the differences between the magnitudes of certain stars used as standards of comparison, Mr. Carpenter's observations and my own agreed; but by using it as an absolute measurer they disagreed as to the angle at which the stars disappeared. On different nights the angle at which the stars disappeared was entirely different, according to whether the sky was clouded or clear, and that must be so.

Mr. Knobel: I have looked very closely into Prof. Pritchard's observations, and had some correspondence with him in criticising and questioning the results. I think there has been one point not brought clearly before the Society, and that is the method adopted in making the observations. I have made many hundreds of observations of extinctions of stars with the method of limiting apertures, and I do not apprehend there is any fundamental difference between the method of extinguishing a star by limiting the aperture, or by that of employing a wedge. But I did find this one certain result: that the operation has to be performed *rapidly*. In extinguishing stars with a slow-moving limiting aperture contrivance, the star disappears at a smaller aperture than with a quick moving apparatus; and that is perfectly explicable. I have questioned Professor Pritchard closely on the method he adopts in making these observations, and I believe that point was never explained to the Society. The operation is performed by sliding the wedge until the star is brought down to a low brightness, and then the screw is rapidly turned, and the whole gist of it is that it should be rapidly turned to extinguish it. Now, by using a limiting aperture instrument, in which I rapidly extinguish the star, I can make the observations consistent; but, if I take a little more time over it, the observations are inconsistent, simply because the pupil of the eye dilates, and for obvious reasons. That is a point which it is extremely important to bring before the Society, because there is the possibility of systematic errors coming in here. We all know how easy it is, in turning a micrometer screw, to unconsciously turn it exactly as much at one time as at another; and I mentioned this point to Professor Pritchard as to the possibility of systematic error creeping in by the wedge being systematically pushed into the same point by the same amount of the twist of the screw, and thus extinguishing the star at the same part of the wedge in consecutive observations; and I cannot get over the point that there may be systematic errors in his method of observation.

Lord McLaren: I may mention, in confirmation of what Mr. Knobel has stated, that having made some experiments with the wedge photometer, on five or six clear nights, I found that, when the wedge was very slowly moved across the field, I was able to follow stars even of the third magnitude to the extreme end of the wedge. They were still visible as very faint points of light; but if the wedge was moved rapidly they were extinguished long before they reached the extreme end. I should not have mentioned this if it had not been confirmatory of observations made upon a much larger scale by one who has given more attention to the subject than I have done. It is evident that this is a thing which must still be investigated, to see whether it is not possible that there is considerable latitude in the estimate of the point of extinction. It seems to me that you cannot get the exact point of extinction within such narrow limits as has been supposed.

The Rev. Father Perry: Would you kindly tell the meeting why Professor Pritchard moves it so rapidly when he has brought the star to a small magnitude?

Mr. Knobel: To extinguish it. (Laughter.)

The Rev. Father Perry: To make the observations accord, I suppose.

General Tennant: Mr. Common and I spent part of a morning with Professor Pritchard, and we asked him about that, and my recollection is that the star was brought to the minimum of visibility, and I look upon that as very much more easy to be observed. But there is another matter, as to the difference between Professor Pickering's comparatively wild results, as some think, and Professor Pritchard's. I believe Professor Pritchard's excellent observations are taken by his assistants, and when he finds they are perfectly discordant, he throws away a whole night's work. My scientific grandfather, as I may call him (Colonel Emerson) taught his pupils, and his pupils taught me, that you ought never to throw away an observation if it was *bonâ fide* made, unless you have a real reason to give against it. But I am not prepared to say mere discordances, under certain circumstances, are not a sufficient reason to throw away an observation if it is presumed that there is a mistake made. I believe that a good deal of the difference in the discordances between the individual results, as given by Professor Pritchard and Professor Pickering, is, if I am right, the result of very different ways of obtaining their results. But taking results, it is marvellous to me that they are so accordant on the whole.

Lord McLaren: I should like to know what is meant by the minimum of visibility. I can understand extinction as applied

to the observation of a star, but the minimum visibility allows a large latitude for personal equation.

Mr. Ranyard: One would expect a very much greater latitude in absolute determinations, such as Professor Pritchard makes from night to night, than in the comparison of two stars on the same night, because in comparing two stars at different altitudes there would probably be less difference in the clearness of the atmosphere in the two directions than between one night and another. I must say the observation of Professor Pritchard, that moonlight does not affect the determination of the brightness of stars, is very surprising when we know how it does affect our estimate with the naked eye and the telescope, and how, on a bright moonlight night, we do not see stars of the third and fourth magnitudes.

Mr. Knobel: The star observed is Polaris, far removed from the moon.

The Astronomer-Royal: That is not a correct statement of fact, because it is not a question whether we see more or fewer stars by moonlight, but a question whether we shall extinguish the stars by the wedge photometer, or by any other means, at the same readings on a moonlight night as on a moonless night. The visibility of stars on a moonlight night is not concerned at all in this matter.

Mr. Howlett: There is a somewhat analogous subject I was testing by means of prisms—candles of incandescent filaments in connection with electrical work, and I should like to know whether it is generally considered that the wedge should be moved rapidly or slowly. I found, practically, that when I brought the wedge to the point nearly approaching extinction I could only just see the filament. I then found, after repeated experiments, that I could see the filament longer by very slowly moving the wedge than by moving it rapidly, and I should imagine the same would be the case in the extinction of stars by Prof. Pritchard's method. With respect to the moonlight affecting the absolute point on the scale of the wedge when a star would disappear, it certainly does seem very marvellous to many of us, because if you carried out the principle logically to the bitter end, I do not see why you should not see stars by sunshine as well as by moonlight.

The President: We have had an interesting discussion, and we are all much indebted to Prof. Pritchard for his valuable paper, for which I am sure we shall all thank him. It is quite a miracle we have had this discussion to-night, because the paper only arrived while I have been sitting at this table.

The Rev. S. H. Saxby said he had hoped to have been able to

exhibit an electrical observatory lamp, but it had not arrived. It was a small incandescent lamp, supplied by a secondary battery, and arranged to hook on to the button-hole. Another thing he desired to mention to the Society: he was about to take a holiday in the High Alps, and had supplied himself with a spectroscope calculated to do good work, and ranging from A to well below the H line. He had arranged for a series of observations, at an altitude of 10,400 feet, on the *Schwarzhorn*, and had already sent out a six-inch equatorial, to be used at an elevation of 5,300 feet. He should feel glad if those who had paid attention to the spectroscope in high altitudes could supply him with any hints that would enable him to make his observations as an amateur of real use.

Mr. Green explained some drawings of Jupiter which he had made, and desired to call attention to some very remarkable spots following the old well-known red spots. He had made one or two remarks about them in the *Observatory*. The spots appeared to be following the red spot in the same latitude, and were apparently of the same kind; and if they should enlarge to anything like the proportion of the red spot, it would be interesting to know how they started. They appeared to begin in a fine dark line, and then enlarged to a claret-coloured line. The larger one, which followed the old red spot at an interval of an hour-and-a half was distinctly oval, and red, rather deeper than the old red, but not so brilliant. It was surrounded by a line of light on the equatorial side, furthest from the south pole, similar to the old red shown in his other drawings. He might say that for twenty years he had made careful drawings of Jupiter, and he hoped by degrees to know something about the question. The colour in the drawings now exhibited had been put on in the dull light of the observatory, so that in the strong light it would appear a little exaggerated.

Mr. Todd: We have made some careful drawings of Jupiter in South Australia, and I hope, during my stay in England, I shall be able to submit them to the Society for what they may be worth. The dark spots to which Mr. Green has referred we saw very distinctly at Adelaide.

Mr. Ranyard: The great red spot seems to have a whitish patch in the middle, with a reddish line round the edge. There are somewhat similar drawings made 15 years ago in America.

The President: I am happy to be able to announce that we have with us an old Fellow of the Society, and an old colleague of my own, who has only arrived in England two days ago. He has been 30 years absent from England, and is no other than Mr. Todd, Postmaster-General of South Australia, and Govern-

ment Astronomer of that Colony. We shall be very glad if Mr. Todd will come forward and give us some little account of the work they are doing in South Australia, particularly in connection with astronomy. (Applause.)

Mr. Todd: When I came to this meeting this evening, I did not expect to be called upon by the President to make any remarks with regard to our small doings in South Australia. Having received an Astronomical education in your grand National Institute, the Royal Observatory at Greenwich, I did not forget my first love. I had to struggle in a young colony with a great many difficulties, and for many years, although I was styled Government Observer or Government Astronomer, I did very little else than carry on, as far as I could with the small assistance and means at my disposal, meteorological observations. But I seized on the proper time, I think, for inducing the colony to make a start towards the establishment of an observatory by availing myself of the Transit of Venus in 1874. I called the attention of the Government to the circumstance, and pointed out that South Australia, as an English colony, should certainly do something towards the advancement of astronomical science, and I made a recommendation, which, I am happy to say, the Government of the Colony and Parliament endorsed, that an equatorial should be purchased for the observatory; and I succeeded in getting that instrument into working order about one night before the Transit of Venus occurred. I was pleased to know that this, my early attempt to do some good service to astronomical science, was successful; and I believe that in the official reports the observations made by myself of egress—I was unfortunate in not being able to get the ingress, through clouds—but my observations of egress came very close to the truth when the whole of the observations over the world were taken into account. I ought to have said, however, that I had availed myself of a previous opportunity of making a start towards the foundation of an observatory. A vexed question arose between South Australia and Victoria, as to where our boundary was. It was described in the Imperial Act as the 141st meridian. Now meridians on the earth are not like those on a map or globe. They are not marked down so that we know exactly where they are, but their position can only be fixed by careful astronomical observations; and there was reason to believe that the eastern boundary line of South Australia determined and marked on the ground in 1847, was considerably to the west, and that it encroached, therefore, upon the dominions of South Australia, and the Government asked me to determine where the boundary line was. At that time it became necessary to mark the common

boundary line of New South Wales, and South Australia, north of the river Murray, which is also the 141st meridian. The two Governments desired that the boundary should be fixed as accurately as possible, and the matter was referred to Mr. Smalley the Government Astronomer of New South Wales and myself. At that time I was erecting a line of electric telegraph to connect directly the capitals of Adelaide and Sydney, along the river Murray independent of the line to Melbourne, and I recommended that voltaic signals should be exchanged between a temporary observatory near the boundary and the Melbourne and Sydney observatories. I had no transit instrument, but fortunately Mr. Ellery, the Astronomer of Victoria, was able to share a very excellent transit made by Simms, which, at my request, he very kindly placed at my disposal, for the purpose of fixing the boundary line; but that accomplished, I so far encroached upon his kindness that I have retained the instrument to this day, and it enabled me to make a commencement in taking transit observations for the determination of time, and dropping a time-ball at the semaphore near Port Adelaide, by means of an electric current transmitted from the Observatory in Adelaide. Having succeeded in getting an equatorial—a very excellent instrument by Cooke & Son, of York—I let the Government have no peace until they gave me a transit-circle; and they have recently built a transit-circle room, which is about 34 feet long, and three or four other rooms used as offices, or computing rooms. It is one of the finest transit-rooms in the southern hemisphere. I had, only just before I came away, succeeded in getting this really splendid instrument into good adjustment. There is a peculiarity in its construction, to which I should like to draw the attention of the Society, and have your opinion. It is virtually a large theodolite—that is to say, the bases upon which the piers rest consist of three parts. First, the sole plate, which is bolted to granite blocks, forming the foundation; secondly, a plate which by means of antagonistic screws has an azimuthal motion; thirdly, a frame, by means of which the piers are levelled. The instrument being adjusted, these three pieces are securely bolted together. I was obliged to resort to an expedient of this kind because of the very large earth movement with which I had to contend. I may briefly say that the transit-circle is an instrument having an object-glass of six inches clear aperture, the divided circles are 36-inches in diameter. It is a very splendid piece of work, and I should like to bear my testimony to the excellent work turned out by Mr. Simms. Another matter to which I may refer is my success in observing the instant of egress of Venus at its last transit in

December, 1882. Expeditions to the east coast of Queensland, as you will remember, were fitted out by the Imperial Government, and my friend Mr. Russell established stations on the Blue Mountains, and the coast of New South Wales, and at Lord Howe Island. Unfortunately it was cloudy at all those stations, and at not one of them I think was there an observation made. The sun would be very low down at Adelaide, so I decided to go to the east, and entering, as it were, the back door of New South Wales, I went up to Wentworth, on the junction of the Darling and Murray rivers, and was there favoured with a clear sky, so as to secure a good observation of internal contact, at a point on the earth which turned one end of the base line. My observations were forwarded to my friend Mr. Stone, and I only hope they will be of some use. Another work in which we have been engaged will possess some interest in the opinion of this Society, and I trust much practical value to geographers, and particularly in connection with the transit of Venus observations. I allude to the determinations of Australian longitudes. Availing ourselves of the kind and valuable services of Captain Darwin, Mr. Ellery, Mr. Russell, and myself have recently been able to make a careful determination of the difference of longitude between Singapore, Port Darwin, Adelaide, Melbourne, and Sydney, by means of the electric telegraph. The Government of Netherlands-India, at my instance, placed an observer (Captain Helb) at Banjoewangie; Captain Darwin occupied Singapore; and Mr. Barrachi, selected by Mr. Ellery, was sent to Port Darwin. The results have been presented to the Colonial Governments in a joint report by Mr. Ellery, Mr. Russell, and myself, a copy of which will shortly reach your Society. Up to the present time, although I have the instrument I have described, I do not know that we have done any great astronomical work of much value. We have paid special attention to Jupiter, and I think, as most of you know, we have been successful in seeing some of the satellites of Jupiter through the edge of the planet. But a great deal of our work is confined to meteorology, and we have a very complete system of observations. I publish daily weather bulletins, with isobar maps. I keep six of these always exhibited, showing them for six succeeding days; and I think I have taught the people of Adelaide to predict the weather for themselves. They are posted, not only in Adelaide at the General Post Office, but also at Port Adelaide and other places where they are useful to the shipping. They show at a glance the varying atmospheric pressure, not only over Australia, but over New Zealand, and they show in a most remarkable manner the progressive march from west to east of the areas of low pressure. (Applause.)

The following papers were taken as read :—

Professor A. Hall : *Observations of the Satellites of Saturn and of the Companion of Sirius, made at the United States Naval Observatory, Washington.*

A. Marth : *Ephemerides of the Satellites of Saturn,* 1885-86.

A. Marth : *Ephemeris for the Satellite of Neptune,* 1885-86.

J. R. Hind : *On Daylight Occultations of Aldebaran in* 1885.

REPORT of the ASTRONOMER ROYAL to the BOARD of VISITORS of the ROYAL OBSERVATORY, GREENWICH.

Read at the Annual Visitation of the Royal Observatory, 1885, June 6th.

We extract a few particulars from this interesting report. The new building and dome for the Lassell telescope are completed. The large mirror, however, requires continual re-adjustments as the telescope is moved, owing to want of stability of its optical axis. Under proper adjustment the definition appears to be very good. The Lee 6-inch equatorial has been transferred to the Hong Kong observatory. The subjects of meridian observations have been, as usual, the sun, moon, planets, and fundamental stars, with other stars from a working catalogue, which now contains about 2,750 stars. About 380 stars have been lately added to the list from the "Harvard Photometry," with a view of making the forthcoming Greenwich catalogue of stars down to the sixth magnitude as complete as possible. It is hoped that all these stars will have been sufficiently observed by the end of 1886, when it is proposed to form a ten year catalogue, epoch 1882·0. The annual catalogue of stars observed in 1884 contains about 1,370 stars. The value found for the colatitude from the observations of 1884 is 38° 31′ 21″·91, differing only by 0″·01 from the assumed value. The mean error of the moon's tabular place (computed from Hansen's lunar tables with Professor Newcomb's corrections) is + 0·02s. in R.A., and + 0″·29 in longitude, as deduced from 104 meridian observations in 1884.

On January 1st the public clock at the observatory entrance, and the other mean solar clocks, were put forward twelve hours, so as to show Greenwich civil time, starting at midnight and reckoning from 0h. to 24h., which would correspond with the universal time recommended by the Washington Conference. The change from astronomical to civil reckoning has also been made in all the internal work of the observatory, and has been carried out without any difficulty ; but the introduction of civil time into the printed astronomical observations has been deferred to allow time for a general agreement amongst astronomers to be arrived at.

For the determination of motions of stars in the line of sight, 569 measures have been made of the displacement of the F line in the spectra of 47 stars, and 72 measures of the *b* lines in 14 stars, besides measures of the displacements of the *b* and F lines in the spectra of the east and west limbs of Jupiter, and of the east and west ansæ of the rings of Saturn. The observations of the last twelve months confirm the change in the motion of Sirius, which now appears to be approaching the sun at the rate of about 20 miles a second.

There were only two days in the twelve months ending 1885, May 20th, on which the sun's disc was observed to be free from spots. It would seem that the maximum both of sun spots and faculæ occurred about the end of 1883 or beginning of 1884. For the year 1884 Greenwich photographs are available for measurement on 152 days, and Indian photographs filling up the gaps in the series on 159 days, making a total of 311 days out of 366 on which photographs have been measured.

The magnetic elements for 1884 are: approximate mean westerly declination, 18° 8′; mean horizontal force, 3·931, in English units; mean dip, 67° 29½′. In the year 1884 there were only five days of great magnetic disturbance.

The number of hours of bright sunshine recorded by Campbell's sunshine instrument during 1884 was 1,115, which is about 100 hours less than the average of the seven preceding years. The aggregate number of hours during which the sun was above the horizon was 4,465, so that the mean proportion of sunshine for the year was 0·250, constant sunshine being represented by 1. The rainfall in 1884 was 18·0 inches, being about 7 inches below the average of the last 40 years.

The errors of the Westminster clock have been under 1s. on 50 per cent. of the days of observation, between 1s. and 2s. on 29 per cent., between 2s. and 3s. on 10 per cent., between 3s. and 4s. on 7 per cent., and over 4s. on 4 per cent.

The personal establishment, the names in each class being arranged in alphabetical order, consists of Mr. Turner, chief assistant; Mr. Criswick, Mr. Downing, Mr. Ellis, and Mr. Maunder, first class assistants; Mr. Hollis, Mr. Lewis, Mr. Nash, and Mr. Thackeray, second class assistants. The Astronomer Royal thus refers to the retirement of Mr. Dunkin:—"During the past year the observatory has lost the valuable services of Mr. Dunkin, who retired on August 25th, after an honourable service of forty-six years, which has been throughout characterised by remarkable zeal and ability, and has contributed largely to maintain the credit of the observatory. Mr. Dunkin has been succeeded in the post of chief assistant by Mr. H. H. Turner, B.A., of Trinity College, Cambridge." Of the assistants generally Mr. Christie writes: "The record of work done during the past twelve months is perhaps the best testimony to the zealous spirit with which the assistants have laboured to sustain the credit of the observatory."

The report concludes with the following general remarks:—"During the past year the various classes of work carried on in this observatory have been somewhat extended. The meridian observations are more numerous than usual, and various subsidiary investigations involving considerable labour have been undertaken with a view to increase their accuracy. A large number of spectroscopic determinations of star-motions have been obtained; and the long-continued maximum of sun-spots has made the photographic measurements and computations much heavier than in any previous year. Extensions have also been made in the magnetic and meteorological branch, which appeared very desirable, but which have pressed rather severely on Mr. Ellis and his staff.

"Turning to the future, I wish to invite the attention of the visitors to the circumstance that an increase in our optical means is required to enable us to carry out satisfactorily the determinations of proper motions of stars in the line of sight with the spectroscope, a work which peculiarly belongs to this observatory, as supplementing the determinations of proper motions from meridian observations.

"The aperture of our largest refractor (12¾ inches) is too small to allow of our observing successfully with the spectroscope any but the brightest

stars; and though the Lassell reflector is somewhat more powerful, its mounting and clock-work are not adapted to carry a heavy spectroscope with the necessary steadiness and accuracy of motion. The firmness of the mounting of the south-east equatorial, and the perfection of its clock-work would make it peculiarly suitable for this class of work if it carried a much larger object-glass.

"After careful consideration of the conditions, I have satisfied myself that an object-glass of 28 inches aperture, and of 28 feet focal length, could be mounted on the south-east equatorial in place of the present object-glass of less than half that aperture; and I have ascertained that Mr. Grubb would be prepared to undertake the construction of such an object-glass, with a tube suited to the special requirements of the case, so that the telescope would be equally available for eye-observations or for use with the spectroscope. With Mr. Grubb's assistance I have prepared a model showing how this may be arranged.

"While a large refractor is required specially for spectroscopic observations, it seems desirable also on other grounds that this observatory should possess an equatorially-mounted telescope comparable with those of other first-class observatories, so that we may no longer be prevented by deficient optical means from obtaining complete series of observations of comets and faint satellites.

"W. H. M. CHRISTIE.

"Royal Observatory, Greenwich,
"1885: May 21."

This suggestion of the Astronomer-Royal is of the highest importance, and all must earnestly desire that it may be practically taken up without delay, for some time is required to produce such a glass. Assuredly the advancement of astronomy could not be better promoted than by furnishing the able and zealous observers of our national observatory with a telescope adequate to the requirements of the present day, and which, we may be confident, would in their hands be unceasingly turned to the best account.

SELENOGRAPHICAL NOTES, JULY, 1885.

By THOS. GWYN ELGER, F.R.A.S.

It would ill become the writer of these notes to omit a reference to the loss which Selenography has sustained by the death of the Rev. Prebendary Webb, whose works have undoubtedly tended more than those of any other English amateur astronomer, to promote and popularize the study of the physical features of the moon. Only those who are old enough to remember the condition of observational astronomy in this country a quarter of a century ago, can fully appreciate the contrast between that time and the present as regards the number of telescopes, of amateur observers, and of literature bearing upon the subject. No British periodical devoted to amateur work (with the exception of the *Monthly Notices*) existed, and those books to which the observer could turn for guidance or advice, were either notoriously behind the

day or far too expensive for the generality of mankind. The appearance of Webb's "Celestial Objects for Common Telescopes," in 1859, marks an epoch in the literature of our science. Though only some 57 pages of the book were specially devoted to the moon, in this, the first edition of it, all that at that time was known to be noteworthy in connection with the larger formations, was described in a style, which, for graphic conciseness, has never been surpassed, while the 12in. map accompanying it with its careful delineations of 404 distinct objects, exclusive of the grey plains or seas, added in no small degree to its usefulness, and rendered it an indispensable handbook for lunar students. Mr. Webb's service to Selenography did not, however, end here. Besides being an active coadjutor of the late Mr. Birt on the Lunar Committee of the British Association, he contributed articles to the *Intellectual Observer*, *The Student*, and other periodicals, on a variety of lunar subjects, in which picturesque and accurate descriptions combine to render them both pleasing to read and permanently valuable. The most prominent characteristics of his work as a lunar observer are its precision and thoroughness. He never abandoned a formation till he had, so far as his optical means would allow, endeavoured to understand the character and significance of the various minor details it included. Though of late years, through age and other causes, he was prevented from taking so active a part in lunar observations as formerly, letters, both public and private, show that his zeal for the progress of knowledge relating to the physical features of our satellite remained as persistent as ever.

Alpetragius d. East of the fine ring-plain Alpetragius lies a region including many features of considerable interest, among which is the large bright anomalous spot *d*, specially referred to in a letter from the late Herr Julius J. F. Schmidt, of Athens, to Mr. Birt, published in *The Student*, Vol. II., p. 48, accompanied by reproductions of drawings of it by Mädler, Lohrmann, and Schmidt. In Beer and Mädler's map of the neighbourhood a well-marked crater, 5 miles or more in diameter, is shown, with a smaller crater on the south side of it, the former being described in *Der Mond* as shining with a light of 8°. In the letter, dated June 5th, 1868, Schmidt writes: "This crater *d* now no longer exists, but in its place a round spot of light, more than ten miles broad, extremely brilliant, which has quite the character of the light spot Linné, and of the few others of this kind which are also found upon the moon. The small neighbouring crater south of *d*, which Mädler gives, is still distinctly visible." This communication, strange to say, seems to have received little attention from observers at the time; and, except very briefly in

Neison's moon, has not, I believe, been subsequently noticed, though the object to which it relates affords as probable an instance of recent change as any to be found on the moon's surface. Under all suitable angles of illumination, Alpetragius *d* has now the appearance of a large circular white spot, very brilliant towards the centre, with an ill-defined nebulous border traceable on the south as far as the small companion crater shown by Mädler and others. While gazing at it, whether with high or low powers, it seems utterly inconceivable that such an object could possibly have been set down as a crater by so experienced an observer as Mädler, unless we suppose it has undergone considerable modifications since his time. At 9^h on 23rd May, the E. longitude of the morning terminator being about 26°, I examined *d* and the neighbourhood with a power of 350. The central portion of the light area was very bright, but not uniformly so. There appeared to be two large oval-shaped nuclei side by side, which were obviously more brilliant than any other portion of the spot, and were at times beautifully defined. No traces of a rim, or any indications of the object being now a depression, were seen, either on this night or on many previous occasions when it has been under examination. It is perhaps a circumstance worth noting in connection with the present peculiar aspect of Alpetragius *d*, that the neighbourhood includes a very irregular group of crater-like objects adjoining the brilliant crater C, about midway between *d* and Lassell, and two remarkable light-surrounded craters west of the latter formation (B and *b*, *Neison's Map XIII.*). B is one of the finest examples on the moon of this class of object. Though the actual nature of the bright glittering nimbus encircling these craters is, and will probably always remain, uncertain and indeterminable, its symmetrical arrangement with respect to the object it surrounds, its gradual degradation in brilliancy from its centre outwards, and its nebulous ill-defined borders, are all appearances which tend to support the hypothesis that it represents material of some kind ejected from the crater with which it is associated. A systematic study of a selected number of the smaller types of lunar craters, supplemented by carefully executed drawings, would possibly throw light on the matter, and settle the vexed question as to whether changes of this character are still going on.

Kempston: June 19th, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE CORONIDS.

Between May 7th and 18th, 1885, I observed 34 shooting stars in observations extending in the aggregate over 7 hours. These phenomena were unusually rare during the period alluded to. Only one radiant point could be ascribed with certainty, and that was close to α Coronæ. I registered 8 meteors belonging to this shower, but for the most part they were very faint, with slow motions and short paths. The exact place of the radiant was at $231^\circ + 27^\circ$.

This little constellation is the well-defined source from whence a considerable number of showers spring during the first half of the year, but whether they are all independent of each other, or whether the point of radiation near α Coronæ is persistent, I cannot say, as my observations in this special case are not sufficiently full or exact to enable me to speak with confidence. The display is, however, more than usually active, and deserves watching through its successive reappearances. In January, 1868 and 1869, it appears to have been exceptionally rich, and a large number of its meteors were observed by Guiseppe Zezioli, at Bergamo. On the morning of January 28, 1868, he records a fine shower of 77 meteors per hour, and this was the chief radiant, supplying 25 paths out of 61 actually registered in $1\frac{3}{4}$h. He also saw fine showers from Corona on January 18 and 30, 1869, and on January 31, 1868. Mr. Greg rightly considered this a very important stream, and in his general table of meteor showers ("B. A. Report on Luminous Meteors, 1876," pp. 156–163) has grouped the positions as follows:—

Jan. 18 to Feb. 13	... $230^\circ + 30^\circ$	11 radiants.
April 12 to June 30	... $235 + 23$	8 radiants.

During the 10 years that have now nearly elapsed since the publication of his catalogue many additional observations have become available for comparison, and I have collected them into a list with the idea of exhibiting their resemblances of position, and possibly of detecting the epochs of greatest intensity. But the various radiant points, as fixed by the different observers,

are not very consistent, and the principal dates of the shower cannot be certainly assigned from the observations, which prove the display is intermittently prolonged over a very lengthy period. The following include all the determinations I have seen between R. A. 221°–238 and Dec. +24–36 :—

Date.	*Radiant Point.* α δ	*Observer.*
December 13	... 230° + 33°	Denning (suspected).
January 18	... 232 + 36	Schiaparelli and Zezioli (No. 8, 21 meteors).
Jan. 21–Feb. 23	... 228 + 29	Denning (from Tupman's obs.).
January 28	... 236 + 25	Schiaparelli and Zezioli (No. 20, 25 meteors).
January 28–29	... 233 + 34	Greg and Herschel (No. 18).
January 30	... 225 + 34	Schiaparelli and Zezioli (No. 22, 15 meteors).
January 31	... 221 + 28	Schiaparelli and Zezioli (No. 24, 14 meteors).
February 13	... 232 + 30	Schiaparelli and Zezioli (No. 33).
February	... 234 + 32	Denning (No. 20, 1877, 12 meteors).
March	... 232 + 26	Corder (No. 12, 12 meteors).
March 27	... 238 + 24	Schiaparelli and Zezioli (No. 44).
Mar. 31–Apr. 12	... 235 + 25	Denning (Ital. obs., 1872, 19 meteors).
April 2	... 230 + 26	Schiaparelli and Zezioli (No. 48).
April 9–12	... 236 + 34	Denning (from foreign obs., 24 meteors).
April 9–12	... 226 + 25	Denning (from foreign obs., 13 meteors).
April 13	... 231 + 27	Schiaparelli and Zezioli (No. 54).
April 20–24	... 230 + 26	Heis (4 meteors).
April 30–May 1	... 237 + 35	Schiaparelli and Zezioli (No. 65, 17 meteors).
April–May	... 230 + 29	Greg and Herschel (No. 67).
May and June	... 232 + 30	Greg and Herschel (No. 67).
May 7–18	... 231 + 27	Denning (8 meteors).
May 16	... 238 + 36	Schiaparelli and Zezioli (No. 67).
May 22	... 232 + 25	„ „ (No. 70).
June 2	... 227 + 30	„ „ (No. 77).

The mean radiant is at $231^{\circ}\cdot5 + 29^{\circ}\cdot4$ from the 24 showers, but it is impossible to say how many different streams the observations may represent. I have excluded several obviously

independent systems at about $230^\circ+17^\circ$, $241^\circ+24^\circ$, $223^\circ+42^\circ$, and $234^\circ+47^\circ$; and there are others near, visible at different times, chiefly in the spring months, which are very liable to become confused with the true *Coronids.*

The radiant points summarized in the table form a series of positions clustering close around α Coronæ, and the mean position lies 3° N.N.W. of that star. Some of the individual places are doubtless affected by considerable ranges of error, and further observations will have an interesting outcome as affording an additional means of investigating this active stream. It should be specially looked for before sunrise on January 18 and 28. It is quite possible that this system, though furnishing us with occasional showers of tolerable brightness, forms another instance of a long-enduring radiant point; but this cannot be demonstrated unless by many further observations of very exact character.

W. F. DENNING.

Bristol: June 9, 1885.

REVIEWS.

British Rainfall, 1883. *On the Distribution of Rain over the British Isles during the Year* 1883, *as observed at more than* 2,000 *Stations in Great Britain and Ireland.* With articles upon various branches of rainfall work. Compiled by G. J. Symons, F.R.S., Secretary of the Meteorological Society, &c., &c., &c. London: E. Stanford, Charing Cross, 1884. pp. 194. Price 10s.

Mr. Symons is glad to say that there seems considerable reason to hope for soon having a trustworthy self-recording rain-gauge for £10. At present, he thinks that from their great cost there are not twenty in the whole kingdom. A legacy of £100 from Miss Nunes enabled thirty new gauges to be issued. One of them is located in Fowla, or Foula Island, which belongs to the parish of Walls, in Shetland, but lies sixteen miles W.S.W. of the nearest part of the Shetland mainland. It measures three miles in length, and one and a half in breadth. The highest peak, called the Kaim, is 1,300 feet high. The frontispiece of this volume is a map of the great rainfall of Sept. 29th, when at one station the fall exceeded four inches. The year 1883 added one more to the unparalleled succession of wet years, which commenced with 1875 . . . . . Back to 1726 there is no instance of more than *five* successive wet years, until the present group, which now consists of *nine.* The largest rainfall was at the Stye, Cumberland, 190·28 in.; the least rainfall at Drem, Craigielaw, Haddington, 18·25 in. The laborious and careful compiler of this annual volume has now devoted twenty-five years to the work. There are, he says, abundant statistics to determine the rainfall for upwards of a century before he began. This work stands still for want of £200 or £300 per annum for additional assistance, and could be completed, he thinks, in about three years, since a large proportion done by himself has already been classified and tabulated uniformly. "Is it," he asks, "to be completed, or to be left for some hypothetical successor?"

An Essay on the Origin and Development of the Solar System. By Ernest R. G. Groth, M.D. London: John Bale and Sons, 87 and 89, Great Titchfield Street, Oxford Street, W., 1884. Pp. 34. 1s.

We always regret, when reading works like this one, that the author had not consulted, before publication, some competent astronomer, and taken the advice which would probably have been given. Whilst sympathising with some of the introductory remarks, we are sorry to be unable to commend the pamphlet itself. The author thinks that as the planets were successively formed out of the sun, the mass of the latter being diminished, the former receded to a greater distance. Thus the earth must have been thrice subject to sudden recessions from its primary at the creation of Venus, Mercury, and Vulcan, which caused as many glacial epochs. Dr. Groth need not to have apologised for his English, which is really very good for a foreigner, and he is to be praised for the spirit in which he writes: "I cordially invite the closest criticism of the principles I enunciate, so that, should I have advanced an unsound theory, it may be speedily exploded, and I, as a lover of truth, may be enabled to rejoice over the pricking of yet another scientific bubble." Preliminary calculation, if not the general consideration of the enormous mass of the sun, would have shown that his attractive power would be very slightly lessened by the loss of the matter of the planets. Those interested in the subject should read some articles by M. C. Wolf, *Les Hypothèses Cosmogoniques*, in the *Bulletin Astronomique** for September and October of this year (1884), where the different theories of Kant and of Laplace are explained in detail.

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
July 1	+35 26 E.	July 11	—86 49 E.	July 21	—29 1 M.
2	23 13	12	+80 59 M.	22	41 14
3	11 0	13	68 45	23	53 28
4	— 1 15	14	56 31	24	65 41
5	13 28	15	44 19	25	77 54
6	25 42	16	32 5	26	+89 53 E.
7	37 55	17	19 52	27	77 39
8	50 8	18	7 38	28	65 26
9	62 22	19	— 4 34	29	53 13
10	74 35	20	16 48	30	41 0
				31	28 47

Moon nearest the Earth, July 11, 14h.
,, farthest from ,, ,, 24, 21h.

List of large telescopes (correction).—Prof. Lewis Swift says that the object-glass of the Van Duzu (not Van Derzu) telescope, which is a dialyte, is 16-in. in diameter. Mr. Van Duzu's son has an observatory for it in Lancaster, a few miles east of Buffalo. It is in use.—*English Mechanic*, July 4th.

* Paris: Gauthier-Villars.

ASTRONOMICAL OCCURRENCES FOR JULY, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Wed	1	14 37 15 59	Occultation of B.A.C. 7774 (6) Reappearance of ditto			Vega. 11 52·3
Thur	2		Sidereal Time at Mean Noon 6h. 42m. 45·26s.	3rd Oc. R.	10 35	11 48·4
Fri	3		Sun's Meridian Passage 3m. 56·40s. after Mean Noon	4th Ec. R.	8 28 40	11 44·4
Sat	4					11 40·5
Sun	5	0 25 14 40	☾ Moon's Last Quarter Near approach of μ Piscium (5)			11 36·6
Mon	6					11 32·6
Tues	7		Saturn's Ring: Major axis=37″·50 Minor axis=16″·77			11 28·7
Wed	8					11 24·8
Thur	9	9	Conjunction of Moon and Mars 3° 7′ N.			11 20·8
Fri	10	11	Conjunction of Moon and Saturn 4° 7′ N.			11 16·9
Sat	11	17 15	● New Moon			11 12·9
Sun	12					11 9·0
Mon	13	3	Conjunction of Moon and Venus 5° 22′ N.	3rd Sh. E. 2nd Oc. D.	8 27 8 50	11 5·1
Tues	14	19	Conjunction of Moon and Jupiter 3° 7′ N.	1st Oc. D.	9 38	11 1·2
Wed	15		Illuminated portion of disc of Venus=0·942 Illuminated portion of disc of Mars=0·951	1st Tr. E.	9 15	10 57·2
Thur	16		Sidereal Time at Mean Noon 7h. 37m. 57·05s.			10 53·3

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Fri	17	2	Conjunction of Venus and Mercury 0° 11′ S. Sun's Meridian Passage 5m. 52·34s. after Mean Noon			Vega. — 10 49·4
Sat	18	12 19 10 10 10 27	☽ Moon's First Quarter Occultation of *m* Virginis (6) Reappearance of ditto			10 45·4
Sun	19					10 41·5
Mon	20	10 58 12 1	Occultation of o^2 Libræ (6) Reappearance of ditto	3rd Sh. I. 3rd Tr. I.	8 57 9 31	10 37·6
Tues	21					10 33·6
Wed	22	10 31 11 1	Occultation of 29 Ophiuchi (6) Reappearance of ditto	1st Tr. I. 2nd Tr. E.	8 55 9 4	10 29·7
Thur	23			1st Ec. R.	9 9 54	10 25·8
Fri	24					10 21·8
Sat	25	19	Conjunction of Mercury and *α* Leonis 0° 12′ N.			10 17·9
Sun	26	14 22	○ Full Moon			10 13·9
Mon	27		Saturn's Ring: Major axis=38″·00 Minor axis=16″·82			10 10·0
Tues	28					10 6·1
Wed	29			2nd Tr. I.	8 57	10 2·2
Thur	30			1st Oc. D.	8 9	9 58·2
Fri	31			2nd Ec. R. 1st Sh. E.	7 49 44 8 22	9 54·3
AUG. Sat	1					9 50·4

THE PLANETS FOR JULY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	7 3 33	N.27 17	5″·0	0 19·2
	9th	8 5 20	N.21 48	5″·4	1 2·9
	17th	9 11 40	N.17 38	5″·8	1 29·5
	25th	9 58 34	N.12 47	6″·4	1 44·8
Venus ...	1st	7 50 28	N.22 21½	10″·2	1 11·5
	9th	8 31 42	N.20 23½	10″·3	1 21·1
	17th	9 11 42	N.17 49	10″·6	1 29·6
	25th	9 50 20	N.14 44	10″·7	1 36·6
Uranus ...	4th	11 58 5	N. 0 59½	3″·8	5 6·6
	20th	11 59 54	N. 0 47	3″·6	4 5·5

Mercury sets about half an hour after the sun, at the beginning of the month, the interval increasing.

Venus about an hour after the sun throughout the month.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor 11, Angel Court, Throgmorton Street, London, E.C.

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The Astronomical Register.

No. 272. AUGUST. 1885.

*OBSERVATORIES OF EUROPE AND AMERICA.**

OBSERVATORY OF SANTIAGO (Chili). We have before laid under contribution parts of MM. André and Angot's interesting work, and we propose now to avail ourselves of it for some account of South American observatories, beginning with that of Santiago, the founding of which is entirely due to the Government of the United States, at whose expense the first observatory was constructed, and the instruments purchased with which observations were made for a long time. An American astronomer was also its first director. From the year 1843 the study of astronomy received an immense impulse in the United States. The Washington Observatory was founded about that time; and to Captain Gilliss, who was one of its original promoters, and its second director, is also due the founding of the observatory at Santiago. It happened thus: in a letter dated 17th April, 1847, Dr. C. L. Gerling, professor at the university of Marbourg, recommended to the attention of Gilliss simultaneous observations in both hemispheres of the planet Venus, with a view to obtaining a better value of the solar parallax than that deduced by Encke from the transits of 1761 and 1769. He begged him to endeavour to obtain the co-operation of American astronomers, so that a sufficiently extended terrestrial base, connecting the observatories of Europe with Washington, might lead to a precise value of the parallax.

* *L'Astronomie pratique et les Observatoires en Europe et en Amérique* &c., &c. Par C. André et A. Angot. Quatrième partie. *Observatoires de l'Amérique du Sud.* Etablissements Météorologiques des Etats-Unis. Paris: Gauthier-Villars, 1881.

This idea much pleased Gilliss, who saw in it a means of determining the sun's parallax by *data purely American*, and he was, moreover, desirous of renewing the attempt, made at the Cape by Henderson in 1832, of applying observations of Mars in opposition for the same purpose. He succeeded in enlisting the interest of the learned societies of America in his ideas. The *American Philosophical Society*, and the *American Academy of Arts and Sciences*, recommended his project to the government, and Congress voted 5,000 dollars (25,000 francs) for the expenses of the expedition, subsequently adding a new credit of 6,400 dollars (29,240 francs). Gilliss was thus enabled to procure two equatorials, one of 4 in., by Fraunhofer, which had been employed by Captain Wilkes in his voyage of exploration; the other by Fitz, 6·5 in. apert., furnished with clock-work movement and micrometers, a meridian circle by Pistor and Martins, with telescope 6 in. aper., and two graduated circles, each read by two microscopes, an astronomical clock, three chronometers; and lastly, a complete set of magnetic and meteorological instruments.

Captain Gilliss, with his assistants, arrived at Santiago towards the end of October, 1849. The authorities at Chili, who received him very kindly, placed at his disposal three stations. After mature examination, he chose the little hill of Santa Lucia, a porphyritic rock about 200 feet high, on the N.E. boundary of the city. The instruments were located just below the summit on the N. side, 175 feet above the streets of the city, and 1,940 feet above the sea level. The astronomical work began in the first days of December, 1849, was continued without interruption till September, 1852.

At the same time Mr. Ferguson was to have made corresponding observations of Venus and Mars with the 9 in. equatorial at Washington. Unfortunately, the weather there was unfavourable, and many series of measures made by Gilliss have remained useless in consequence of the want of simultaneous observations at Washington and the other observatories concerned. Thus, for 139 observations of Mars by Gilliss, only 11 were made at Washington, 5 at Cambridge (Mass.), and 4 at Greenwich. For 78 observations of Venus by Gilliss, only 8 were made at Washington. The principal object of Gilliss was therefore not attained, the elements for discussion being too few. But he had also made a complete review of the southern heavens; and the observations of zones by his assistants and himself are of great importance. They give the positions of about 27,500 stars between the equator and the parallel of 65° of south declination, and of 290 double stars, of which a great many were new.

Before the departure of Gilliss, on 1st October, 1852, the

government of Chili had purchased all the *matériel* of the expedition, including even the books which had been brought. The observatory of Santa Lucia thus became the national observatory of Chili, and was placed under the direction of Dr. Charles Mœsta, a graduate of the University of Marbourg (Germany).*

The government of Chili wished to make the observatory of Santiago a real scientific establishment as a school associated with the *National Institute.* To this end it maintained two pupils of the institute every year in the observatory. This plan, however, which was pursued until 1859, does not appear to have attained all the results anticipated. M. Mœsta could not find an assistant, and was forced to make all the meridian observations and their reductions by himself. After he had, by means of the moon and some zenithal distances approximately determined the position of the observatory, he undertook the review of the portion of the heavens between the parallel of — 62° and the zenith of Santiago; but after the observation of some of the zones he found that such a work required more than one observer. He therefore contented himself with re-observing such stars of the old catalogues (*British Association Catalogue, Catalogue de la Caille*), whose positions were doubtful, or which appeared to have a considerable proper motion. He discovered that many of the proper motions of southern stars given in the catalogue had no existence, but were due to errors in the catalogue of la Caille. M. Mœsta gave his attention also to the great and small planets, and made with the equatorial many series of observations of the minor planets and comets, the results of which appeared in the *Astronomische Nachrichten* of 1853 to 1860.

In the course of his varied work M. Mœsta had discovered that the instruments which would have been thought absolutely stable, as resting on the rock itself, were by no means so. The rocks in the vicinity of the small observatory buildings were heated in an extraordinary manner during the summer, and in consequence the meridian circle was in a state of perpetual movement, so that the variations in azimuth exceeded 0·5s. in twelve hours. Moreover, since the arrival of Gilliss, the city of Santiago had been much enlarged. The hill of Santa Lucia, at that time at the eastern boundary of the city, was now almost in the midst of it, so that the frequent sound of the bells of the innumerable

* The results of the expedition of Gilliss have been published in six volumes. Vol I. contains the geography of Chili, its climate, government, social condition and resources, mineral and agricultural. II. Report of Lieutenant MacRae on the Andes and Pampas, various works on natural history. III. Observations of the planets Mars and Venus. IV. and V. Meridian circle observations of stars and zones. VI. Magnetic and meteorological observations.

clocks of the capital, and the neighbourhood of different military quarters, perpetually disturbed the observer at his work. The government of Chili having consented to the transfer of the observatory to a more favourable site, the works were begun in April, 1857, but were not terminated till May, 1860.

The new observatory, situated at Yungai, a suburb of Santiago, is a very plain building, about 160 feet in length, lying east and west. In the middle is a small tower with a dome, intended for the equatorial of Fitz; on each side are the rooms for work and the library; and at the extremities of the building, east and west, two small meridian rooms, one containing the meridian circle of Pistor and Martins, the other the transit instrument. To the north, the edifice is extended in the direction of its axis; after the central tower is a square room intended for a transit instrument in the prime vertical, and beyond is a second dome for Fraunhofer's equatorial.

M. Mœsta and his assistant, M. Valckman, who had been at the observatory since the end of 1856, at once commenced their labours. M. Mœsta resumed the study fruitlessly entered upon by him in 1856 of the star α of the centaur, with the view of determining its parallax. From October 16th, 1860, to May 28th, 1864, he made 216 observations of the zenith distance of α Centauri, and 202 of the neighbouring β Centauri. Assuming that the parallax of the last-mentioned is nothing, the comparison of corresponding observations leads, for that of α Centauri to the value $0''.88 \pm 0''.068$. Th. Maclear at the Cape of Good Hope had obtained $0''92$. The two results are therefore quite in accordance.

At the same time the astronomers of Santiago resumed the observation of zones between $-40°$ and $-45°.30'$ declin., extended as far as stars of the 9th mag. In 1865 there had been obtained about 10,000 positions of stars. They re-observed almost all the stars of la Caille's cat. between $-50°$ and $-60°$ declin., as well as the stars of the B.A.C. remarkable for their large suspected proper motion, and a certain number of circumpolar stars. Moreover, in response to desires expressed on the one part of M. Winnecke, and on the other by MM. Pierce, Gould, and Gilliss, M. Mœsta carried out at the opposition of Mars in 1862 two series of observations of that planet, one with the meridian circle, the other with the equatorial of Fitz. These observations were published at Santiago in 1863, and their combination with those made at Washington and Upsala led M. Hall* to adopt for the solar parallax the value $8''.84$.

* Washington Astronomical and Meteorological Observations for 1863. Introduction, p. xlv. and foll.

We must also record, amidst the numerous labours of M. Mœsta at Santiago, the series of observations on comets undertaken from 1860 to 1865; and, above all, the discovery of the great comet of 1865, which he made with the naked eye on the 18th July, at the baths of Callina, 20 miles N.E. of Santiago la Cordillère, very shortly after sunset, and through a fine twilight. Drawings of this comet, and the elements of its orbit, will be found in No. 1,519 of the *Astronomische Nachrichten.* This was the last work of M. Mœsta. Shortly after he obtained from the Chili government unlimited leave to return to Europe for the recovery of his health, to study the progress made since 1852 by the European astronomers, and to cause to be constructed for the observatory of Santiago the instruments which that progress rendered needful. Thus he soon despatched to Chili a large equatorial telescope by M. M. Repsold, of Hamburg, the objective of which, by Merz, is 9·8 in. apert., a self-registering barometer and thermometer, and a set of magnetical instruments of the best construction.

But serious difficulties, and in particular his enfeebled sight, prevented M. Mœsta from returning to Chili. He soon resigned his functions, and settled at Dresden, where he occupied himself with the reduction and preparation for publication of all the observations made at Santiago from 1860 to 1865, which it is greatly to be wished, in the interest of the science, that the government of Chili would decide upon printing. The former observations have been all published in two volumes, which appeared, one in 1869, the other (at Dresden) in 1875. Since the installation of the new observatory a regular series of meteorological observations have been made, which have been separately published. The successor of M. Mœsta was a Chilian astronomer, M. J. Ign. Vergara, who had been an assistant to M. Mœsta from 1859 to 1865.

[M. Mœsta died at Dresden, after a severe illness, on the 2nd of April last, in the fifty-ninth year of his age. See a notice of his life and work in "The Observatory," June, 1884.]

Observatory of Cordoba (Argentine Republic). Whilst the labours of Lalande, Bessel, Weisse, Argelander and Carrington had produced complete and accurate catalogues of the stars between the north pole and 31° south declin., the southern heavens remained almost unexplored. Halley, on his return from St. Helena, had published a catalogue of 341 southern stars reduced to the epoch 1677. Nearly a century later F. Baily published the positions of 9,766 stars observed at the Cape by la Caille, and reduced to the beginning of 1750. Later appeared the catalogue of Brisbane, 7,385 stars observed at Paramatta

(New South Wales) by S. Rumker and J. Dunlop, and reduced to 1835, a certain number of which belonged to the northern hemisphere. About 1883, Johnson, one of the ablest of observers, published a catalogue of 306 southern stars observed at St. Helena for epoch 1845. Lastly comes the catalogue made from 1843 to 1847 by Taylor, at Madras, containing the positions of 11,015 stars, of which, unfortunately, only about 6,000 belong to the southern hemisphere. To these purely stellar catalogues must be added the catalogues of nebulæ and star clusters to which John Herschel devoted so many years.

But besides that these different catalogues embraced a very small number of stars (12,000), barely a twenty-fifth of all the stars useful for the continued observations of erratic bodies, no thorough comparison had been made of them with each other, and the errors even of the oldest had never been determined with certainty and completeness. Argelander, who had undertaken at Bonn the complete review of the northern heavens, had often expressed a wish that a similar work should be executed for the southern hemisphere. This had been undertaken by Gilliss in his famous expedition, but the inefficiency of his equipment, which was designed for another object, the want of assistants, and the effect of the climate of Santiago, which hastened his departure, had greatly restricted his work.*

The question was discussed in 1862 by the Royal Astronomical Society of London. A committee composed of MM. Airy, Hind, and Carrington, was charged by the Society to take measures for the realisation of the plan for observing at least 300,000 stars, each one not less than three times, to the 9th mag., and three years after the committee was enabled to announce to the R. A. S. that three observatories in the southern hemisphere would undertake this review on a uniform plan. These were the observatories of Madras, for all the portion between the equator and 40° declin., that of Melbourne, for the zone of — 40° to 80°; and the Cape observatory for all the polar region to 10° from the pole. At the same time an astronomer of Boston, M. B. A. Gould, formerly director of the Dudley observatory, sought the means of accomplishing the idea put forth by Argelander, and advocated by the R. A. S. Struck with the advantages presented by the climate of Cordoba, half-way between the Atlantic and the Pacific, and the immunity of the place from cyclones and

* A first catalogue, comprising 1,963 stars from the zones of Gilliss, was published in 1871, at the expense of the Government of the United States, by Professor Harkness, astronomer of the Washington observatory The entire work of Gilliss gives the positions of 29,403 stars, between the south pole and the parallel of — 24°

earthquakes, he wrote in 1865 to M. Sarmiento, then minister of the Argentine Republic, to ask the protection and support of the Argentine government for his projected astronomical expedition, the funds of which had been promised by friends of science in America. He asked whether, on his departure from Cordoba, the government would take over his establishment, and continue it as a national institution. The reply of M. Sarmiento was very cordial, but circumstances at that time hindered the plan. When, however, M. Sarmiento became, in 1868, President of the Republic, one of his first acts was to propose to the Congress the erection of an observatory. The proposal was almost immediately adopted, and towards the end of 1869 M. Gould received a letter from M. Avellanada, minister of Public Instruction, in which the Argentine government charged him to instal a permanent national observatory, and to furnish it with the necessary instruments.

Fortunately, at the time of his first attempt in 1865, M. Gould had ordered, on his own responsibility, from the celebrated Repsold, of Hamburg, a meridian circle of the same power as had been used by Bessel and Argelander, having a telescope 54 in. focal length, and 4·5 in. aperture. This instrument was now almost finished. There was also in the workshops of Alvan Clark an excellent objective by Fitz, 11 in. aperture which Gould purchased, caused it to be mounted as an equatorial, and ordered a clock from Tiede, of Berlin. He had also a small equatorially mounted telescope, 4 in. apert., and the Coast Survey of the United States placed at his disposal all the portable instruments which he might require. Moreover, the American Academy of Boston conceded to him the funds of the Rumford Prize, in order that he might construct an apparatus for studying the light of the stars; he thus obtained a stellar photometer of Zöllner, and an excellent spectroscope by Merz and Mahler. The Smithsonian Institution, the Naval Observatory, the Bureau of the *Nautical Almanac*, the principal scientific societies of England, the Observatories of Greenwich, Pulkowa, and Leipzig, and most of the astronomers of Europe, contributed to form the library of the new establishment. At the same time M. Gould received the co-operation of four young students from the Universities of Philadelphia and New England, who were to form the first nucleus of the Observatory of Cordoba.

Everything being thus arranged, M. Gould left New York on May 28, 1870, and reached Buenos Ayres, by way of Europe, on August 25, 1870. He proceeded at once to Cordoba, which is on the western limit of the Pampas, and resolved to establish the observatory on a hill about 120 feet high, about a mile from the city. The war between France and Germany, and the

yellow fever which prevails so often at Buenos Ayres, greatly retarded the arrival of the meridian circle, so that regular observations did not commence before September, 1872. M. Gould had occupied his enforced leisure in making charts and a catalogue of stars visible to the naked eye at Cordoba—that is, from 10° north of the equator to the south pole. Argelander, in 1843, had executed a similar work for the northern hemisphere (*Uranometria Nova*), and in a space including about eight-tenths of the heavens, had found 3,256 stars from the 1st to the 6th mag. Afterwards M. Heis took up the work again, first at Aix-la-Chapelle (1845), and later at Münster. His catalogue, published in 1872, contains for a portion of the heavens, little different from that of Argelander, 3,139 stars from the 1st to the 6th mag.* Adopting the number of M. Heis, we may conclude with sufficient probability that the northern half of the heavens contains 4,909 stars from the 1st to the 6th mag. The number of stars visible evidently depends on the purity of the sky where they are observed; now, at Cordoba, such is the transparency of the sky, that the catalogue of M. Gould, to which he has given the title of *Uranometria Argentina,* includes 7,670 of such stars for the southern half of the heavens. In reality, the *Uranometria Argentina* contains 8,522 stars, of which 852 are comprised between the equator and 10° north declination.

In the observations of zones, commenced at the end of 1872, M. Gould endeavoured to make his work independent of previous catalogues. To this end each zone is accompanied by the observation of clock and circumpolar stars, intended to fix the absolute positions of the stars embraced. The zones the most full of stars contain 293, the mean number is 140, which makes about one star for an interval of twenty-one seconds of time. Observations by eye and ear could not attain this rapidity; all the right ascensions are observed chronographically, as is almost universal in the present day. An assistant reads the microscopes of the circles, inserts the magnitude and group of each star, and the approximate time of observation, in order to facilitate the ulterior combination of these results with those given by the chronograph. Although the climate of Cordoba did not entirely answer the expectations of M. Gould—the number of clear nights being not much greater than in the United States—the ardour and zeal of the observers were such that at the end of the first year 429 zones, containing 56,280 stars, had been observed, and on the 8th September, 1875, the work, which M. Gould had

* The *Novus Atlas Celestis* of M. Heis includes in reality the positions of 4,701 stars; but that astronomer did not stop at the 6th mag., but went as far as the 6·6 magnitude.

proposed to himself, was quite finished. The southern heavens between the 23° and 80° of declination had been reviewed, 754 zones observed, and rather more than 105,000 observations made. As to the polar spherical calotte, ten degrees in extent, which had been neglected, that had been completely studied by Gilliss at Santiago, and Stone at the Cape of Good Hope, so that M. Gould thought he might consider it as sufficiently surveyed by those observations. To render the zone observations entirely independent of former catalogues, the original intention contemplated a list of about 3,000 stars, each observed four or five times with all possible care. But when M. Gould went to the United States in 1874, the zone work was necessarily interrupted, and to occupy the leisure of his assistants, he resolved during his absence that the meridian circle should be employed for the formation of a much more considerable catalogue of fundamental stars. This work was also continued after his return, so that now the catalogue includes the positions of near 10,000 stars, veritable celestial bench-marks, by which the places of all the rest are fixed.

Besides those labours which had led to his sojourn at Cordoba, M. Gould had a strong desire to carry out others of not less importance and utility. We have elsewhere* described the ingenious methods employed by M. Rutherford for the photography of groups of stars, by which the most delicate measurements are possible, and the right ascensions and declinations obtained with great exactitude, which would require an immensely longer time by direct measuring. M. Gould had been a sharer in these labours, and was the first to submit to calculation the results of these measures, and to determine the relative positions of the principal stars in the groups of the Pleiades and of Præsepe. To enable him to continue these fine researches, M. Rutherford had entrusted him with a large telescope furnished with two different objectives, one for direct observation, the other for photography. But if photographic observation saves much time, it demands a considerable outlay, and all the funds of the observatory had been absorbed by the other labours; it was, moreover, necessary that M. Gould should have a practised assistant. His family connections supplied the money required, and M. Rutherford sent him a young photographer of New York, whom he had trained himself. On unpacking, however, the photographic objective, it was found that the flint lense was broken in two, and it was necessary to order another from M. Fitz, of New York. Other hindrances delayed the work; but on

* See the third part of this work, *Observatory of M. Rutherford*, p. 148, *et seq.*

September 8, 1875, seventy good proofs had been obtained, giving the images of 24 different stellar clusters, and of various double systems. On one of these proofs, that of the cluster H. 323, are seen 102 stars. M. Heard, who succeeded the former photographic assistant, also obtained photographs of Mars, on which the details of the surface of the planet are almost as well defined as in direct observation; he has likewise been able to take proofs of the telescopic planet Eurydice (75), and of companion stars, with a view to assist in the system of observations recommended by M. Galle for obtaining a correct value of the sun's parallax. This is an original idea, and deserves to be followed out. We have not yet heard the results of which it may be capable. It must be added that direct measures are made with the equatorial in order to obtain by another means the relative positions of the stars composing the different stellar groups which are studied photographically.

We will conclude the list of the astronomical labours at the Observatory of Cordoba by stating that M. Gould has himself also co-operated, by observation of the planet Flora, for the determination of the sun's parallax; and that by observations made concurrently with M. Vergara, at Santiago, and M. Mœsta, at Buenos Ayres, he has determined the differences of longitude of these three points. Lastly, seventeen meteorological stations have been established in the Argentine Republic, which, with however limited instrumental resources, already render great service, by collecting information on the climate of that part of the world, which hitherto has been completely wanting. The *personnel* of the Observatory of Cordoba at present consists of MM. John Thomas, Heard, and Eug. Bachmann, assistants; and MM. Latzina and Bigelow, computers.

(*To be continued.*)

ON SOME STARS PROBABLY VARIABLE.

By J. E. Gore, F.R.A.S.

Among the large number of stars which have been suspected of variability in light the following seem to be especially worthy of attention, as almost certainly variable, although their periods and limits of variation have not yet been determined. The positions are given for 1880·0, and observations are much to be desired. They may all be observed with a good opera-glass.

1. Lalande 47304 Ceti=Lacaille 9732, 0h. 1m. 36s., S. 23° 11′. Rated $6\frac{1}{2}$ by Lacaille and Lalande, 6 mag. by Behrmann, but not given by Argelander or Heis. It was estimated 5·9 mag. at Cordoba, but was measured only 7·29 with the photometer at Harvard (H.P. 2). The star may possibly be a variable of the type of Algol, the measurement at Harvard having perhaps been made at or near the time of a minimum.

2. Lalande 2798 Ceti 1h. 25m. 29s., S. 7° 20′. Rated 7 mag. by Lalande, and estimated 6·9 mag. at Cordoba, where it was suspected of variation. On November 12, 1884, I estimated it 8 mag., and on December 21 found it *below* 8 mag. Assuming the Cordoba estimate to be correct, this star would seem to be certainly variable. It lies *n.p.* the 6 mag. star Lalande 2848 (*n.f.* θ Ceti).

3. Lalande 7172 Tauri, 3h. 46m. 45s., N. 7° 25′. Rated $8\frac{1}{2}$ by Lalande, and 7·8 mag. in the *Durchmusterung*. At Cordoba the estimates varied from 6·8 to 7·9, and the star is probably variable to the extent of about a magnitude, with perhaps a long period. I estimated it about 8 mag. on the following dates, September 30, 1883; November 9, December 21, 1884; and February 3, 1885.

4. O. A. 5270 Canis Majoris, 6h. 28m. 7s., S. 27° 51′. Rated 6 mag. by Heis (No. 18 of Canis Major), but only $8\frac{1}{2}$ and 9 mag. by Argelander. It was estimated 9 and $9\frac{1}{2}$ mag. at Cordoba, but was measured 5·74 with the photometer at Harvard Observatory (H. P. 1240). On the evenings of February 14 and 16, 1885, in a very clear sky and no moonlight, I failed to see any trace of the star with a powerful binocular field-glass, although other small stars in the vicinity were distinctly visible. Comparing the Harvard measure and Heis' 6 mag., with the Cordoba estimates and those of Argelander, the star would seem to be a remarkable variable, and deserving of careful observation, but it lies very low on the English horizon, and satisfactory observations must be looked for from more southern latitudes. The star is not given by Harding or Behrmann.

5. Lacaille 2470 Canis Majoris, 6h. 45m. 18s., S. 27° 12′. This is another southern star which is probably variable from about 6 mag. to 8 mag. It was rated $6\frac{1}{2}$ mag. by Lacaille, 7 mag. Harding, 7 and $7\frac{1}{2}$ mag. Argelander, 6 mag. Heis, 8·2 mag. Johnson, 7·5 mag. Yarnall, 7 to 8 mag. at Cordoba, and 6·53 mag. at Harvard. On February 14, 1885, in a very clear moonless sky, I estimated it 7·2 mag. by careful comparison with neighbouring stars.

6. τ^4 (17) Serpentis, 15h. 30m. 54s., N. 15° 30′. This is

No. 356 of Birmingham's Catalogue of Red Stars, and Birmingham's observations vary from 6 mag. to invisibility in his 4½-in. refractor. It was rated 6 mag. by Lalande, and 7·5 mag. by d'Arrest. It was measured 6·89 at Harvard, and "estimated" 6·1 (H.P. 2599). The star seems certainly variable. I have found it sometimes brighter and sometimes fainter than a star closely south of it (Lalande 28460); but my observations are not sufficient to determine the period or limits of variation.

7. V. Capricorni, 20h. 10m. 5s., S. 21° 41′. One of Sir J. Herschel's "ruby" stars, and rated by him 6½ mag. It is 7½ mag. in Lalande's Catalogue (38339) and was rated 7 mag. by Argelander. Webb called it 6 mag., September 3, 1873; Birmingham, 7·5 mag., July 14, 1876. In July, 1875, I estimated it only 8½ or 9 mag., and 8½ mag. in November, 1876, and August 1877. Secchi suspected variation, and rated it 7·5 mag. in July, 1868, and 7 mag. in September, 1869. The star is evidently variable, like many of the red stars, and it should be watched by southern observers.

8. Anon. Capricorni 20h. 23m. 51s., S. 12° 38′. Variously estimated at Cordoba from 6·8 to 8½ mag. It is not in Lalande's Catalogue, or Harding's Atlas. On September 13, 1884, I found it much fainter than Lalande 39361 (rated 7·6 mag. at Cordoba), and therefore below 8 mag.

9. 8122 B.A.C. Cephei, 23h. 13m. 13s., N. 73° 2′. Lionville announced the variability of this star in 1856 (*Comptes Rendus*, Vol. XLII. p. 546), but it does not appear that the variation has been confirmed by any subsequent observer. Lionville's observations show a variation from 6 mag. to 10 mag., with a probable period of 115 days, "but very doubtful owing to want of sufficient observations." The star seems to be identical with No. 4040 of Groombridge's Catalogue, where it is rated 6 mag. It is not given by Heis. It was rated 7 mag. by Franks in November, 1877, and suspected of variation. I have 16 observations of the star between February, 1884, and March, 1885, and my estimates vary from 7½ to 8 mag. It may possibly be an irregular variable with a long period.

10. 19 Piscium, 23h. 40m. 15s., N. 2° 49′. This star is certainly variable, but the exact period is not yet quite certain. It seems to vary from about 4·8 to 6·2 mag., and from observations 1880–1884, Espin deduces a period of 165± days with a well-marked maximum of 5·2 mag. on August 19, 1884.

SELENOGRAPHICAL NOTES, AUGUST, 1885.

By Thos. Gwyn Elger, F.R.A.S.

The Rill-system of Ramsden. The very curious group of clefts found in the neighbourhood of Ramsden is especially interesting to those who possess the optical means necessary to examine it as a whole, and are lucky enough to hit upon a suitable time, as regards position and definition, for the scrutiny. In Schmidt's *Rillen* (*Tab. II.*) fifteen distinct clefts are shown. The majority of these, however, are difficult objects, especially those lying on the western side of the ring-plain. In fair observing weather five clefts may be easily distinguished, even with small apertures, when the east longitude of the morning terminator is about 40°, viz., two on the south and three on the north side of Ramsden, all of them running up to the wall of the formation. They are very evidently a connected system formed subsequently to the ring-plain on which the individual clefts abut. Neison (*Moon*, p. 378) remarks: "The origin of the system of rills appears to be without the ring-plain, which seems to have entirely intercepted the system, as several of the rills disappear at its walls, and reappear beyond, without interfering with either the walls or the interior of Ramsden." Though the bright level interior of the ring-plain shows no signs of being traversed by these surrounding clefts, Neison's assertion as regards the border exhibiting no indications of disturbance by them is not correct, inasmuch as their continuation through the walls, albeit in a very attenuated form, is traceable under high powers and good definition. From 8h. to 10h. on 29th October, 1884, five clefts were easily visible abutting on the border of Ramsden. The interior was filled with shadow and the shadow of the brilliant eastern wall extended to about one-fifth of the distance between this wall and the bright crater Ramsden *e* (see *Neison's Map XIV.*). Though definition was not first rate, a power of 350 on my 8½-in. Calver-reflector exhibited the grosser details of the border and the clefts with remarkable distinctness. Three of the latter on the north of Ramsden, lettered 9, 10, 11 in Schmidt's *Rillen*, were by far the most obvious. The most westerly of the three, 11, apparently originates at a brilliant little mountain in the hilly region bordering the S.W. side of the Mare Humorum, and, curving very slightly towards the east, runs up to the north wall of Ramsden which it evidently traverses in the form of a narrow gap, not half the width of the cleft beyond the formation. The two broad clefts east of this, 9 and 10, appear to have a common

origin in the same mountain region; but, gradually separating as they proceed southwards, they strike the north wall at two points about five miles apart. The more westerly (10) of these was also traced over the wall as a fine dark line, but the companion cleft (9) was involved in shadow before reaching it, and consequently its course up to the border could not be traced. Of the two long clefts south of Ramsden, that which abuts on the west wall was the more distinct. It forms a part of the remarkable system which, speaking roughly, radiates from a common point, some twelve miles south-west of the ring-plain. This was also traced over the wall as a more delicate cleft, but was only seen thus by glimpses, owing to unfavourable air. It would be well for those who command large apertures and good observing stations, to endeavour to confirm Dr. Klein's very interesting and suggestive observations of these Ramsden clefts, which I can only do in a very imperfect fashion. With respect to the two clefts 9 and 10, just noticed, he has remarked that that portion of the wall of Ramsden included by them is considerably disturbed, and shifted from its normal condition towards the exterior. This observation was made when the morning terminator was only slightly to the east of the ring-plain, and, of course, with high powers in exceptionally good air. Its confirmation would tend to establish the fact that Ramsden itself existed previously to the clefts which environ it, and would possibly give a clue to the succession of formations in the immediate neighbourhood. Careful sketches of the ring-plain and its surroundings show clearly enough that clefts 1 and 7 are simply prolongations of 10 and 9. (*See Neison's Map XIV.*) Attempts should be made to trace 10 and 1 across the floor. The failure, however, to discover them in this position, will not lessen the probability of Ramsden being of more ancient date than the clefts, as it is quite possible that the interior may have been covered with material since the clefts were formed and all signs of them obliterated, though they are obvious enough under high powers on the slopes and summit of the circumvallation.

There is an unusually brilliant oval spot on the inner slope of the north-east wall of Ramsden which is unnoticed by Selenographers, and the bright crater *e* on the east, is drawn far too small in proportion to the diameter of the ring-plain in all published maps.

Kempston: July 20, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

STELLAR PHOTOMETRY.

SIR,—In the July number of the *Register*, you give a report of a somewhat animated discussion by the members of the R. A. S., on a communication of mine to the Society on "certain points relating to Stellar Photometry." That paper was not intended by me for an extempore discussion, because I was aware that it referred to topics, some of which were not obvious, and others required a deliberate consideration; otherwise it would have been my duty to have been present at the reading of the paper. The Society, however, is greatly indebted to the Astronomer Royal for the admirable explanations which he gave to the several difficulties as they were successively propounded by some of those who took part in the discussion. So far, my absence was really a gain to the listeners; and beyond that, his valuable remarks are now accessible to the readers of the *Register;* nevertheless, as there are a few points which still require explanation, I shall request a little space whereby I may hope to remove any erroneous impressions which may still possess the minds of astronomers interested in the subject. I need hardly say that it will be desirable for the reader to have at hand the last numbers both of the *Monthly Notices*, and of the *Register*.

Mr. Knobel, and some others with him, owing I fear to some inadequate explanations of mine, both personal and written, have been, it seems, led to the impression that by means of a certain screw attached to the photometer, the observer with a rapid and possibly habitual movement of the screw might continuously move the wedge up to the same point, which he had persuaded himself, was the point of extinction of the star. I cannot help thinking the suggestion is at least as ingenious, on the part of the objector, as it would be on the part of the observer to succeed in such an act of unconscious legerdemain. But as he would virtually change his instrument, by altering the aperture of his telescope, it would not lie within the range of probability that he could make his two sets of independent measures accord in the way they ought, and actually do, accord. Be this as it may, there is a still more cogent reason for dismissing all doubts as to the

mysterious action of this screw, for Mr. Plummer very rarely, if ever, uses the screw; and no such screw is attached to the photometer used by Mr. Jenkins. Intention and attention, combined with sufficient experience in the use of this extremely simple instrument, are the only secrets in the production of the accordant results.

I regret that the evidence for the point of extinction of Polaris being unaffected by moonlight, was received with some incredulity; but the evidence of the fact is overwhelming, as demonstrated in the course of many thousand extinctions of the star in every phase of the moon. Moreover the cause of it seems obvious, and it was clearly explained by the Astronomer Royal. To Mr. Howlett, I am obliged for the question as to the quite different effect which would be produced by bright sunlight. His question has led to some experiments of a quite successful character, in relation to the whole subject, and these I may probably communicate to the Society at a future period. It is in results of this sort that the utility of the evening discussions may be traced.

General Tennant appears to have been under the impression that occasionally a whole night's measures have been suppressed on account of discordances, and that, unintentionally, I have led him to that misapprehension. I regret it, if I have so misled him, but the reply is sufficiently simple; in vol. xlvii. of the *Society's Memoirs* I have given the results of some twelve thousand extinctions. I have, in connection therewith, suppressed not a single measure. All are equally utilized, but I believe I should have acted more wisely, if I had suppressed several measures taken in unfavourable weather; and in the revision of the work now in course of printing, I shall certainly suppress some, though never without notification.

As to whether or not I have made an improper or injudicious selection from the measures in the Harvard Photometry, I thought and still believe, that I gave the entire table of the Harvard measures of all the brighter stars down to Polaris, taken from page 406 of the Harvard volume. My object was to inquire from the records published in that remarkable and valuable volume itself, whether three determinations of a star's relative magnitude were necessarily sufficient to secure accuracy. The facts of the case lie before the impartial judgment of the reader.

I was also sorry to read the opinion expressed by one of our eminent fellows, to the effect that he did not believe in either the meridian or the wedge photometer. This also was my own mental attitude some four years ago, but long subsequent experience with the latter has entirely changed my opinion. The double image, or polarising photometer, originally devised by Zöllner and

modified in the meridian photometer by Professor Pickering, is a very effective instrument, of extreme beauty. The wedge photometer, as I have modified and applied it, is of extreme simplicity. The probable error of the observations made with the Zöllner photometer is reported by Dr. J. Wilsing, of Potsdam, to be much greater (0·053 mag. as against 0·035 mag.)* than that deduced from the wedge observations; and this is confirmatory of my own experience. Be that as it may, each is trustworthy and valuable in its own line of proper application. But, after all, it is the eye which is the actual photometer; the optical instrument effects little more than to facilitate the means whereby the eye or the brain may record its judgment. Many experiments lead to the conclusion that this judgment is very limited in extent, when applied to light which in its character is either feeble or intense. Photography, no doubt, presents the promise of future usefulness but I fear it will also be attended with similar limitations.

As applied to the stars, the experience of four years leads to the conclusion that either form of photometer, *i.e.*, of the wedge or any form of the Zöllner, properly applied on any single fine night, may be reasonably expected to furnish the apparent relative magnitude of a star with an error not appreciably exceeding the tenth of a magnitude.

I am, sir, your obedient servant,

C. PRITCHARD.

Oxford University Observatory: July 17, 1885.

REVIEWS.

Eighteenth Report of the Board of Visitors to the Observatory; together with the Annual Report of the Government Astronomer. Melbourne, Sept. 1883.

Mr. Ellery's report is, as usual, very satisfactory. Mr. White, the chief assistant, obtained a year's leave of absence to visit Europe, and to recruit his health. The great telescope continues in good condition; and although the inevitable loss of reflective power increases a little year by year, it does not yet affect the work on which it is engaged to any sensible degree. Indeed, some photographs of faint objects obtained lately are clear evidence of the immense light-gathering power it still possesses, and of the trivial loss occasioned so far by the slight tarnish apparent. Arrears of work in publication are being gradually overtaken, the astronomical results and annual catalogues from 1876 to 1880, forming volume VI., are now in the hands of the printer, and the second general ten-year catalogue is also well advanced, but its completion has been delayed by pressure of work and the failing health of the chief assistant, Mr. White. As usual, the meridian observations have claimed first attention. The revision of Sir John Herschel's nebulæ has steadily proceeded, and, according to requirement or opportunity, cometic and planetary obser-

* See Astronomische Nachrichten, Vol. 109. S. 50.

vations for solar parallax, &c., have been carefully attended to. Twenty of the nebulæ and clusters of Herschel's Catalogue have been sketched: some were looked for and could not be found, in some instances probably owing to an unfavourable state of the sky. During the year observations of some kind were obtained on 79 nights; 57 nights were taken up with visitors. On the remaining 229 nights cloudy weather, full moonlight, or some other cause interfered with the work. In accordance with a scheme arranged by Mr. Gill, the Astronomer-Royal at the Cape of Good Hope, a very fine series of measures of difference of declinations between the planetoids Sappho and Victoria and selected fixed stars was obtained on about 30 out of the 90 nights apportioned for the work. Perhaps the most interesting astronomical event of the year was the apparition of the great comet in Sept., 1882, which was sufficiently visible for measures of position up to the 26th April, 1883. From its first appearance, says Mr. Ellery, to the last observation with the great telescope we had it under sight for 250 days. An extensive series of measures were got, as well as some photographs and spectroscopic measures. As regards the transit of Venus, Mr. White secured excellent observations at Hobart, and Mr. Ellery and Mr. Gilbert obtained a satisfactory series of observations; besides, a number of photographs were made by Mr. Turner. The results of the Victorian parties' observations were transmitted to the British Committee. The result of recent telegraphic operations makes the longitude of the Observatory 9h. 39m. 53s·37 E., the seconds hitherto having been 54·8. The branch of meteorology and terrestrial magnetism has been carried on as usual. Intercolonial weather telegraphy is now moderately complete. As regards work done gratuitously for the public may be mentioned the rating or adjusting 30 chronometers and 10 watches; testing 65 aneroid and mercurial barometers; selecting and testing numerous surveying, meteorological, astronomical, and nautical instruments for mariners, explorers, and others. The largest tax on the staff in this direction, however, which increases every year, is in furnishing meteorological and physical data, copies of records, answering questions upon these and astronomical subjects to numerous applicants in this and the neighbouring colonies, or from other countries. A few photographs of the moon, enlarged to 16 inches diameter, ages 9 days, 10 days, and 15½ days respectively, are to be sent to the Calcutta Exhibition.

So soon as the new transit circle is erected and properly adjusted, it will be employed upon the revision of a rather large catalogue of stars at the request of the Astronomische Gesellschaft, besides the special work of the observatory. Mr. Ellery proposes to devote the great telescope as exclusively as possible to the continuation of the revision of Sir John Herschel's nebulæ. He also hopes to arrange for a photograph of the sun being obtained on every *possible occasion.* His desire and exertions that the Observatory "may honourably maintain the position it now holds among others of the world" will, we trust, be crowned with success richly deserved.

Annual Report of the Board of Regents of the Smithsonian Institution, &c., &c., for the year 1882. Washington: Government Printing Office, 1884. Pages xviii. and 855.

This report contains much interesting matter. We learn that 15,560 copies were printed by order of Congress, of which 2,500 were for the Senate, 6,060 for the House of Representatives, and 7,000 for the Institution itself. The Secretary and Director is Professor Spencer F. Baird,

who, we are glad to see, states that "at no time has the establishment been in better condition, whether we take into account the character of the work accomplished, the economy of expenditure, or the satisfactory condition of its funds at the end of the year." Under the auspices of this important Institution valuable explorations are carried on, especially in America; astronomical announcements are made by telegraph, and international exchanges effected with every country of the civilised world. A general appendix of 400 pages gives a scientific *résumé* for the year. The astronomical part of this is by Professor L. S. Holden. Then there are 154 pages of papers relating to anthropology, with maps and illustrations of objects found in the mounds in Illinois, Indiana, and Ohio.

Want of space is felt in the new National Museum building. Amongst other collections of ores, forest timber, &c., the building-stone collection is especially valuable, consisting of many thousands of samples of marble, granite, sandstone, and other substances, for the most part dressed in 4-inch cubes, each of the faces showing a different surface and treatment. The policy of the Smithsonian Institution is to occupy no ground that is covered by other sufficient agencies. A catalogue of its numerous publications, brought down to July 1, 1882, contains 478 titles, and comprises 342 pages 8vo. A general catalogue of scientific periodicals, published in all parts of the world, which will form an octavo volume of between 700 and 800 pages, is in progress. Vol. iv. of the "Proceedings of the National Museum" has been published, forming an octavo volume of 680 pages with plate and figures.

The number of establishments outside of the United States with which correspondence and exchange have been conducted amounts to 3,726, an increase of about 800 over the list of 1881. The number of packages from Europe for distribution in the United States during the yast year amounted to 7,187; the number of packages from the United States for transmission abroad was 19,292, making an aggregate of 26,479 packages. In 1882 the library was increased by the accession of 11,789 books and pamphlets. The numbers for 1880 and 1881 were 8,570 and 11,959 respectively.

The many ramifications of the work of the Smithsonian Institution, its good organisation, the able management of its directors, with its adequate endowment, have greatly benefited science in past years, and there is every prospect that it will continue to do so even more in the future.

A Catalogue of suspected Variable Stars, with Notes and Observations. By J. E. Gore, M.R.I.A., F.R.A.S., honorary member of the Liverpool Astronomical Society. [Reprinted from the proceedings of the Royal Irish Academy, 2 Ser., Vol. iv., No. 3 (Science).] Dublin: University Press, 1885.

This catalogue contains 736 stars (brought up in a supplementary list to 773), followed by notes and observations. The distribution of known and of suspected variable stars is exhibited in a plate of the northern and southern planispheres, red dots marking the known, and black the suspected variables. The catalogue has been compiled from various sources, and the author has given his own observations of a considerable number of the stars, made previously to October, 1877, in the Punjab, India, and the remainder in Ireland. He usually employed a binocular by Browning, object glass 2 in., magnifying about 6 diameters. This glass, he says, readily shows stars to about the 9th mag., or fainter, when not close to brighter stars, and in a clear moonless sky. The field glass was always

accurately focused. Professor Pritchard's magnitudes with the Wedge photometer, and those of the Harvard photometry, have been added. The positions of the stars have been brought up to 1880·0. This valuable work cannot fail to be of great use to those occupied in this department. Mr. Gore observes that many of the stars noted are doubtless really variable, though more observations are required to place their variability beyond question. In some cases the variation would seem to be small, with a long period, possibly several years. Others may possibly belong to the Algol type, and from the nature of the light curve the variability may escape detection for a long time. On the other hand, future observations will no doubt show that many of them are constant in their light, and these, of course, must be removed from future catalogues. Observations of these less suspicious stars should, however, not be neglected, as even negative results will be of value as tending to show constancy of light.

Celestial Motions; a Handy Book of Astronomy. By William Thynne Lynn, B.A., F.R.A.S., formerly of the Royal Observatory, Greenwich. Second Edition. London: E. Stanford, 55, Charing Cross, S.W., 1884. Pp. 78. 1s.

We are glad that so many must have already appreciated this little book by a known master of his subject. Any astronomer will find it to be what it professes, a concise digest of the most important facts which have been discovered regarding the motions of the celestial bodies, and the dimensions of those belonging to our own system. Particular care has been taken to give the most recent available information. For example, there is a complete list of all the minor planets to No. 237, discovered by Palisa, June 27, 1884. We have found it a most convenient work for ready reference.

We read in "La Nazione," Florence, 15th March, that "the following notice appeared in the Astronomical Review, 'Ciel et Terre.'" M. Palisa, astronomer at the observatory at Vienna, the well-known fortunate discoverer of various planets, desirous of obtaining the necessary means for a voyage with the view of observing the total eclipse of the sun which will happen in 1886, wishes to sell the right of giving a name to the asteroid No. 244, recently discovered by himself, for the sum of 1,250 francs. M. Palisa discovered in 1884 six new asteroids, known by the names of Honoria, Celestina, Adrastia, Grimhilda, and Foa, whilst the last of the sisterhood, provisionally inscribed in the celestial civil state under the number 244, is yet awaiting a name. What a fine opportunity for assisting science, at the same time that an original kind of present is made to the gentlewoman whose dear name it is desired to immortalise in the firmament!"

DUN ECHT CIRCULAR, No. 94.

Professor Krueger, of Kiel, telegraphs that a comet was discovered by Barnard (of Nashville, U.S.), on July 7, and was observed at Harvard College, Cambridge, U.S., as follows:—

	Greenwich M. T.	R. A.	Decl.
1885, July 9	17h. 17m. 31s.	17h. 17m. 48s·4.	S. 6° 1′ 8″.

The daily motion was —2m. in R. A. and 35′ towards the south. It is described as not more than 1′ in diameter; 11th magnitude or fainter, and as having some central condensation but no tail.

In terms of the code the message runs :—"Unharmed Barnard ukase unnoticed Harvard Observatory July cartouch keen carbuncled treat constablery rascal smallness unhappy cordage."

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1885, July 13.

DUN ECHT CIRCULAR, No. 95.

The elements and path of the new comet discovered by Barnard as computed by Mr. S. C. Chandler, jun., from observations on July 7, 9, and 11, have been telegraphed from Boston, U.S., as follows :—

Elements.

$$T = 1885, \text{ May } 16{\cdot}72, \text{ Greenwich M. T.}$$
$$\left.\begin{aligned} \pi - \Omega &= 150^\circ \; 8' \\ \Omega &= 91 \;\; 28 \\ i &= 84 \;\; 27 \end{aligned}\right\} \text{Mean Equinox } 1885{\cdot}0.$$
$$\log q = 0{\cdot}39340$$

Ephemeris for Greenwich, Midnight.

1885.	R. A. h. m. s.	Decl. ° ′	Brightness.
July 13	17 10 48	S. 8 10	0·87
„ 17	17 4 4	10 23	
„ 21	16 57 48	12 31	
„ 25	16 52 12	S. 14 32	0·71

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1885, July 16.

THE PLANETS FOR AUGUST.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	10 31 5	N. 8 31	6″·0	1 49·8
	9th	10 58 4	N. 4 12½	8″·0	1 45·2
	17th	11 11 18	N. 1 16½	9″·2	1 26·9
	25th	11 5 47	N. 0 54½	10″·4	0 50·0
Venus ...	1st	10 23 7	N. 11 41	11″·0	1 41·8
	9th	10 59 35	N. 7 55	11″·2	1 46·7
	17th	11 35 14	N. 3 55	11″·6	1 50·8
	25th	12 10 25	S. 0 12	12″·0	1 54·5
Saturn ...	26th	6 25 18	N. 22 25½	15″·6	20 2·4

Mercury sets about three-quarters of an hour after the sun, at the beginning of the month, the interval decreasing.

Venus sets about an hour after the sun throughout the month.

Saturn rises near midnight on the 28th.

ASTRONOMICAL OCCURRENCES FOR AUGUST, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Cygni.
Sat	1		Sidereal Time at Mean Noon 8h. 41m. 1·94s.			11 54·6
Sun	2		Sun's Meridian Passage 5m. 58·20s. after Mean Noon			11 50·6
Mon	3	9 55	☾ Moon's Last Quarter			11 46·7
Tues	4	9	Conjunction of Jupiter and Mercury 2° 32′ S.			11 42·8
Wed	5	15 32 15 56 5 19	Occultation of B.A.C. 1526 (6) Reappearance of ditto Conjunction of Saturn and μ Geminorum 0° 4′ N. Conjunction of Jupiter and Venus 0° 26′ N.			11 38·8
Thur	6	8	Conjunction of Saturn and Mars 1° 20′ N.			11 34·9
Fri	7	2 3	Conjunction of Moon and Saturn 4° 13′ N. Conjunction of Moon and Mars 5° 33′ N.	1st Sh. I.	7 57	11 30·9
Sat	8	5	Conjunction of Venus and Mercury 3° 42′ S.			11 27·0
Sun	9					11 23·1
Mon	10	0 14	● New Moon			11 19·2
Tues	11	14 21	Conjunction of Moon and Jupiter 2° 30′ N. Conjunction of Moon and Mercury 1° 55′ S.			11 15·2
Wed	12	0	Conjunction of Moon and Venus 2° 30′ N.			11 11·3
Thur	13					11 7·4
Fri	14		Sidereal Time at Mean Noon 9h. 32m. 17·15s.			11 3·4
Sat	15		Illuminated portion of disc of Venus=0·881 Illuminated portion of disc of Mars=0·946			10 59·5
Sun	16		Saturn's Ring: Major axis=38″·84 Minor axis=17″·01			10 55·6

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Cygni.
Mon	17	1 46 22	☽ Moon's First Quarter Neptune at quadrature with Sun			10 51·6
Tues	18		Sun's Meridian Passage 3m. 35·42s. after Mean Noon			10 47·7
Wed	19					10 43·8
Thur	20	11 39 12 35 12 8	Occultation of B.A.C. 6287 (6) Reappearance of ditto Occultation of B.A.C. 6292 (6)	The		10 39·8
Fri	21	12 35 1 39	Occultation of ρ^1 Sagittarii (4) Reappearance of ditto	Satellites		10 35·9
Sat	22			of		10 31·9
Sun	23			Jupiter		10 28·0
Mon	24	2	Conjunction of Uranus and Venus 0° 13′ N.	are		10 24·1
Tues	25	5 25 17 9 18 9	○ Full Moon Occultation of 67 Aquarii (6) Reappearance of ditto	invisible		10 20·2
Wed	26	23	Conjunction of Jupiter and Mercury 6° 1′ S.	through		10 16·2
Thur	27	8 48	Near approach of B.A.C. 8365 (6½)	his		10 12·3
Fri	28			nearness		10 8·4
Sat	29			to		10 4·4
Sun	30			the Sun.		10 0·5
Mon	31					9 56·6
SEPT. Tues	1	10 52 10 51 11 1 11 32 11 9 11 21 12 1 13 40	Occultation reappearance of θ^2 Tauri (4½) Occultation reappearance of θ^1 Tauri (4½) Occultation of B.A.C. 1391 (5) Reappearance of ditto Near approach of ν^1 Tauri (5½) Occultation of 85 Tauri (6) Reappearance of ditto Near approach of α Tauri			9 52·6

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
Aug. 1	+16 33 E.	Aug. 11	+74 23 M.	Aug. 21	−47 46 M.
2	4 20	12	62 10	22	59 58
3	− 7 53	13	49 57	23	72 11
4	20 6	14	37 44	24	84 23
5	32 19	15	25 32	25	+83 24 E.
6	44 32	16	13 19	26	71 11
7	56 45	17	1 6	27	58 59
8	68 58	18	−11 6	28	46 46
9	81 11	19	23 21	29	34 34
10	+86 35 M.	20	35 33	30	22 21
				31	10 8

Moon nearest the Earth, Aug. 8, 22h.
,, farthest from ,, ,, 21, 6h.

Books received.—Sirius.—Science Monthly.—Science Observer.—Bulletin Astronomique.—L'Astronomie.—Annals of the Astronomical Observatory of Harvard College. By E. Pickering.—Researches on Solar Heat, &c.—United States Signal Service Papers, No. XV. Washington. 1884.—British Rainfall, 1884. Compiled by J. C. Symonds, F.R.S. London: Edward Stanford. 1885.—Schellen's Spectrum Analysis. Translated by Jane and Caroline Lassell. Edited by Capt. W. de W. Abney, R.E., F.R.S. London: Longmans, Green & Co. 1885.—Once a Month. Edited by Peter Mercer, D.D. William Inglis & Co., Melbourne. London Publishers; Griffith & Farran.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To April, 1885.

Lawton, W. E.

To June, 1885.

Campbell, Sir A.
London Institution.

To Dec., 1885.

Harrison, H.
Lawrence, E.
Rylands, S. G.

To June, 1886.

Gore, J. E.
Tebbutt, J.

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The Astronomical Register.

No. 273. SEPTEMBER. 1885.

TELESCOPIC METEORS.

By Professor Dr. Schafarik, Prague.

Mr. Denning has directed attention to the desirability of recording the paths of telescopic meteors (*Astronomical Register*, p. 227). These bodies have long ago excited the interest of astronomers. Schrœter seems to have been the first to mention them in his *Aphroditographische Fragmente*, p. 241, where he relates, that in 1795, on June 28, at 2 p.m., he saw pass through the field of his 27-ft. reflector (20-in. aperture, power 185, field 15′), in 1s., a faint greyish point of light, quite similar to stars of the same brightness, and of no appreciable diameter. The late T. F. Schmidt, of Athens, devoted to them an interesting chapter in his work, "Resultate aus 10 jährigen Beobachtungen von Sternschnuppen" (Berlin, 1852); in the course of 10 years he noted 146 of them, from 7th to 11th magnitude. Observations of telescopic meteors are also related by Mason, 1839; Carrington, 1853; Winnecke, 1854 (who saw 105 on 32 evenings in a 3-in. finder, magnifying 15 times, field 3° 1′), Luther, Heis, and others. Dr. E. Hartwig, of the Strassburg Observatory, told me in 1881 that he had seen numbers of them during his observations of variable stars. Professor Winnecke had asked him carefully to lay down their tracks in the Bonn Star Maps, and he did so for some time, but was obliged to give it up, the additional work interfering too much with his principal task.

Since 1879 I have been engaged almost exclusively in observa-

tions of variable stars; and in that time I have seen hundreds of meteors of every magnitude, from the 2nd down to the 12th, passing through the field of my 6½-in. silvered glass reflector (ordinary power 32, field 54′), or its 1½-in. finder. To me they are so common that it would be difficult to pass a night at a low power telescope of large aperture without having caught sight of a couple of them. 1880, August 30, I noted in my observing book: "It would be difficult to tell the number of telescopic meteors which passed to-night (between 9h. and 15h.) the field of my telescope or its finder; I think more than 50, if not nearly 100." And on the subsequent night (9h. to 14h.): "To-night also, numerous telescopic and some naked eye meteors seen; less than last night, about 20, many of them only diffused luminosities."

I had no time to register the tracks of these bodies in the Bonn Star Maps, though I am sure that after some practice it could be done with considerable accuracy; I only used to note their number, and such particularities of movement and aspect as I could make out during the short moments of their passage across the field. Since 1883 I was compelled to give them quite up, as I had every effort to make to carry out my programme of variable star observations, for I found soon that every distraction made my comparative estimations of starlight less easy and less precise.

I find telescopic meteors in general can be divided into four classes:—

1. Well-defined star-like objects of very small diameter, round, or of no recognizable shape, sometimes with smoky luminous trails of cometary aspect—*i.e.*, widening as they recede from the principal body.

2. Large luminous bodies of some minutes of diameter, round or ovoid, sometimes pretty well defined, ordinarily diffused and smoky, with a wedge-shaped trail, fading as it recedes from the body.

3. Well-defined discs of a very perceptible diameter, almost invariably brighter at the border than at the centre, which gives them the aspect of hollow transparent shells, or luminous bubbles. Particularly when they happen slowly to travel across the field in horizontal direction, they look like soap-bubbles driven by wind.

4. Faint diffused nebulous masses of irregular shape, considerable size, and different colours.

Meteors of the first class are often coloured—yellowish, golden, deep orange, glowing red; many meteors of a rich green hue have I seen with the naked eye, none in the telescope.

1880, August 5, there appeared in the field of my telescope a fine reddish star-like meteor, 3 mag., stationary for 2·5s. in R.A. 3h. 27·2m. Decl. + 38° 22′ (1880·0). The view of the trail and its movements is highly interesting. I remember particularly one of 1·2 mag. in Perseus, 1880, August 6, and the superb spectacle offered by the ethereal luminous clouds of its bright yellow trail, rapidly driving and fading off amongst the innumerable stars near χ Persei.

An interesting sub-division of class I. comprises faint greyish points of light 9·10 and 12 mag., slowly moving in short paths, sometimes curved, often beginning or ending, or both, quite within the field. For instance, 1881, March 29, 11h., a faint greyish point appeared in the centre of my finder (directed towards κ Corvi), moved slowly towards the left, developing gradually into a sharp white star 9·10 mag., and ended, before it had reached the border of the field (4°). The considerably curved path (of about 1° 40′) turned, in reality, its *convex* side towards the earth, and the time of flight was 1·5s. This was, without a doubt, the foreshortened projection of a very distant long meteor-path; but at other times these grey points are seen at such altitudes above the horizon that we are compelled to place them far beyond the received limits of terrestrial atmosphere, if we suppose that they possess the same velocity and mean length of path as brighter meteors, and appear so dim only from their great distance from us. For instance, 1883, May 6 and 13: "Very faint points of light travelled through the field of the telescope, in Leo, Cancer, Draco; one of them described his short curved path quite within the field." The dim light of these glimmering celestial sparks may be a consequence of the utmost rarity of the medium they encounter; and it is quite possible that after having traversed a shallow segment of the atmosphere they continue their path in planetary space. Mason came to the same conclusion from observations of 50 telescopic meteors, made 1839, August 1st to 10th, by a reflecting telescope magnifying 80 times. He found their angular velocity by no means greater than that of naked eye meteors; and from this he inferred that they moved at enormous distances from the eye, in some cases more than 1,200 miles. Professor Schiaparelli ("Theory of Falling Stars," ch. i., § 2, note) is more inclined to suppose that their small velocity is a consequence of their very small mass and the great relative resistance of air; but he wishes not to deny that some falling stars begin their apparition at least 400 miles above the earth.

In class I. I should place an object of so peculiar a character that I do not know what to make of it. 1874, April 24, at

about $3\frac{1}{2}$h. p.m., I observed the moon (illuminated nearly $\frac{3}{4}$) in bright sunshine, with power 66, field 34′, of a fine 4-in. achromatic by Dancer, when I was surprised by the apparition, on the disc of the moon, of a dazzling white star, which travelled slowly from E.S.E. to W.N.W., and after leaving the bright disc, shone on the deep blue sky like Sirius or Vega in daylight and fine air. The star was quite sharp and without a perceptible diameter.

It is well known that luminous star-like objects are seen in summer-time near the sun. Schwabe has given much attention to them, and called them "Lichtflocken;" it is generally admitted now that they are partly the pappus of various seeds, partly convolutions of gossamer, floating high in the air, and brilliantly illumined by the sun when nearly in the line between sun and eye. So, for instance, I noted, 1877, May 4: "Numerous brilliant flocks near the sun. . . . Some of them moved slowly, and resembled most perfectly white fixed stars of the first magnitude." But I know of no observation of Schwabe's Lichtflocken nearly opposite to the sun. As a displacement of the eye-piece of $\frac{1}{80}$th of an inch makes the image indistinct, the focal length of the telescope being 5 feet, the distance of the object must have exceeded 16,600 feet. The calculated altitude of the moon at 3h. 30m. was 36° 35′ (without regard to refraction and parallax), and so the object must have been at least 9,800 feet above the earth; but in all probability it was higher, for it was absolutely sharp, and its flight so slow (about 5s.) that even a trace of indistinctness would have been perceptible. Old Schrœter has seen something of that kind at night (1789, October 15, 17h.) when scrutinizing the unillumined part of the moon with his 7-ft. reflector, power 161. Suddenly a "splash of light," as he calls it, consisting of small sharp sparks of light, was formed on the disc of the moon, and crossed the rest of disc and field in 2s., and before it had left the field there was formed another splash nearly at the same place, and left the field in the same direction in other 2s. Numerous telescopic bodies, of very small diameter, moving rapidly across the moon, or near it, were also seen by Abbé Ch. Lamey, in 1864 and 1873 (*Les Mondes*, 1873, November 20). Many of them may have been birds or bats; but others, being luminous, and their paths rectilinear, were in all probability meteors.

Objects of the second class are more rare. I shall give a few instances of them:—

1882, June 10, 10h. 55m. A fine meteor of 7 mag. shot in $\frac{1}{2}$s. across the field of τ Sagitarii (in apparent direction 300°). It was bright yellow, round (diameter 2′), diffused, and looked like

a yellow loose snow-ball, followed by a short train of scattered flakes of snow.

1883, April 7, 13h. 40m. A very small point of light appeared in the field of S Cephei, developed in a moment into a large oblong rosy star of 7 mag., surrounded by a faint broad luminous envelope, and followed by a narrow, long, pointed train, which gave to the whole the aspect of a large falling drop of molten liquid. Apparent direction of flight, 180°.

1883, September 3, 14h. 0m. A large diffused irregularly ovoid meteor of 7 mag., and 4′ diameter (estimated by distance of stars in the field), with a diffused, wedge-shaped, smoky tail, shot across υ Tauri, in (apparent) direction 270°.

Objects of the third class are pretty frequent.

1882, March 14, 12h. 30m. A 7 mag. milky white, round, luminous bubble of 1′ diameter shot upwards, near ν Cygni.

1882, May 11th, 10h. 20m. An orange-coloured irregularly ovoid luminous bubble of 7·8 mag., many minutes long, crossed the field of S Libræ, in apparent direction 270°. Its light was irregularly distributed; the brightest part followed.

1882, September 9th, 10h. A faint, large, well-defined luminous bubble of an irregular ovoid outline (axes, 1·2), and unequally distributed light.

1883, May 4th, 13h. A fine meteor, 9·10 mag., in direction 170°; a white bubble of ½′ diameter. Looked as if driven by wind.

Sometimes the shape is much elongated, nearly fusiform.

1882, June 27th, 12h. 30m., near κ Cassiopeiæ. A bright yellow luminous fusiform cloud, 10′ long, 3·4′ broad, was formed in the field, and left it in 1s.

The difference between Classes I. and III. may perhaps be only an apparent one, and may be caused by the smaller size or greater distance of the bodies of Class I. Indeed, I happened to witness a transition from Class I. to III.

1878, April 27th, near δ Geminorum. A meteor entered the field of power 85 (diam. 31′) as a sharp white line of light, but expanded gradually to a white luminous bubble of 10″ or 15″ diameter, with sharp circular outline, brighter at the margin, darker in centre; duration from 2″. to 3″. On two other occasions I happened to see meteors with higher powers, and the impressions obtained are not unfavourable to the hypothesis just mentioned.

1879, March 14th. A white, round, sharply-defined bolide, 4·5 mag., shot across the field of power 108 (near κ Crateris).

1880, August 10th. Another of 7 mag., and glowing red, crossed the field of power 85 (in Aquarius). Both seemed to me

round, of about 5″ diameter, with brighter margin, and comparatively dark hollow centre.

On the other hand, if we try to form an idea of the chemical process going on in meteors of Class III. during their visibility, we are naturally led to suppose that the combustible gases occulted—as we know by the researches of Graham, Mallet, and Wright—in the meteorites, are expelled by the sudden enormous calefaction they suffer on entering the terrestrial atmosphere, and form a burning envelope, which may appear a hundred or thousand times larger than the solid body. The latter, in the majority of cases, may weigh a couple of grains only, as Prof. H. A. Newton has shown by calculation, founded upon their mean velocity and mean luminosity. On the second supposition the difference between telescopic meteors of Classes I. and III. will be a real one, and founded upon scarcity or abundance of combustible gases occulted in them.

Telescopic meteors of Class IV. are the rarest. Schmidt saw none. Winnecke mentions a number of them; some reddish, others blue.

1881, Nov. 23. A greyish elliptic cloudlet of 2′ diameter, tolerably well defined, shot exactly across κ Aquarii. Its brightness equalled that of Herschel's first class of nebulæ, but it looked more compact and less transparent. A very faint diffused nebulosity of this class (diameter many minutes) seen 1880, Aug. 31, in Delphinus, was decidedly reddish.

1882, Sept. 14, 8h. 20m. A faint grey shadow passed over S Persei.

1883, July 28, 11h. A faint grey perfectly diffused vaporous mass, 4′ diameter, originated in the field of κ Camelopardi, and moved in a curvilinear path out of the field.

In our mineralogical museums hundreds of meteoric stones are preserved, and have been thoroughly studied in modern times. They present a great variety of types, from pure, compact iron through hard crystalline silicate rocks to porous friable masses, easily broken with the fingers. The identity of falling stars and meteorites has been doubted by some physicists, but Schiaparelli regards their arguments against identity as insufficient; and so we may admit that the matter which constitutes falling stars is similar to that of bolides. Now if we try to establish a relation between the different known classes of meteorites and our four classes of telescopic meteorites, we may describe it thus:—

I. Solid bodies, small, very compact and refractory, not easily disaggregated by the enormous pressure they suffer on entering the terrestrial atmosphere. Little or no occulted gases. The smoke accompanying part of them may consist of the superficial

melted layer, torn off and dispersed by the friction of the atmosphere. (Hard stony meteorites.)

II. Bodies larger than I., of a less compact material, which is easily melted and torn off by the mighty current of air produced by their rapid flight. Another part of their envelope and trail may consist of vapours and gases. (Tufaceous and conglomeratic stony meteorites.)

III. Small, very compact, and refractory masses, rich in occulted gas, which is expelled by their sudden enormous calefaction, and expands almost equally in every direction, presenting thus the appearance of a ball. (Siderites.)

IV. Clouds of cosmical dust, or meteorites so soft and friable that they are crushed and converted into dust as soon as the pressure of the atmosphere begins to act upon them.

SELENOGRAPHICAL NOTES, SEPTEMBER, 1885.

By Thos. Gwyn Elger, F.R.A.S.

Alpetragius d.—Delineation of Lunar Formations. Since the notice of this object, which appeared in the July *Register*, I have been favoured with a very interesting communication from Mr. Geo. Knott, LL.B. containing the results of observations of the white spot and its surroundings, made with his 7⅓in. refractor in the year 1868, soon after the late Prof. Schmidt had drawn Mr. Birt's attention to the remarkable discrepancies between Beer and Mädler's representation of it, and its aspect in that year. It will be remembered that Schmidt asserted that in the position where Mädler shows a prominent crater, some five miles in diameter and shining with a light of 8°, no crater existed in 1868, "but in its place a round spot of light more than ten miles broad, extremely brilliant, which has quite the character of the light spot *Linné*." This is the object now called Alpetragius *d.* On 5th November 1868, when the west longitude of the evening terminator was about 17° 55′, it was noted by Mr. Knott as "a brilliant soft-edged white spot, which presents no appearance of being a crater." On 23rd December, 1868, however, under morning illumination, the longitude of the terminator being about 25°, a small craterlet, about one half the diameter of that which lies on the south margin of the bright circular area, was detected a little north of the apparent centre of this area. On 20th February, 1869, at 7h. 30m., the east longitude of the morning terminator being 21° 25′, the minute craterlet previously observed near the centre of the white spot, was

very distinct, and as before, appeared to be about equal to half the diameter of III. A[vz] (the small crater on the south margin of *d*). As the latter is certainly under a mile and a half in diameter, Mr. Knott's craterlet is considerably less than a mile across, and cannot answer to the prominent object drawn by Mädler in the same situation, which he estimated to be 5 miles in diameter. What renders the suspicion of change in this region still more probable is the present existence of the brilliant circular area, ten miles or more in diameter, on the site of Mädler's crater, not the slightest indication of which is shown by Beer and Mädler, though they are careful to exhibit the far less noticeable nimbus of light encircling the neighbouring crater Alpetragius B, north-west of Lassell, which lies in the same field under moderate powers.*

It is unfortunate in this, as in many other instances that have occurred in recent times where there has been a fairly-grounded suspicion of change, that it has been impossible to supply evidence sufficiently exact to remove those outstanding doubts which surround the observations appealed to in order to establish the fact. This state of things will continue to exist so long as we have only lunar charts (however excellent) and desultory observations to rely upon. What is really required are exhaustive studies under various conditions of illumination of certain limited regions, which include objects of the type of Alpetragius *d*, Klein's dark craters in Alphonsus, those near Beaumont, the black spots in the neighbourhood of Gambart C, and in other localities, with carefully prepared drawings on a large scale and measurements, accompanied by descriptive notes relating to every object shown. Till this is done, more or less uncertainty must necessarily be attached to every fresh instance of suspected change that is brought under notice. As it was with Linné and Hyginus N, so it will be to the end of the chapter. Sceptics and those who complacently assume that the moon is "a changeless world," will be justified in their opinion that selenographers have not made out a shadow of a case in favour of lunar activity, though, I imagine, there are few who really devote themselves to the observation of the minutiæ of the moon's surface who are not impressed with the improbability of the changelessness contended for, albeit they are unable actually to point to effects produced by forces now in activity sufficiently free from the taint of uncertainty to convince the doubters. An attempt will shortly be made under the auspices of the Liverpool Astronomical Society

* Knott's small crater near the centre of the light area was observed as a minute puncture by Mr. Whitley, of Penarth, Truro, in 1874, and described by him in the *English Mechanic*.

to carry out the above suggestion by selecting definite areas of very moderate size, and, after observing them in detail under as varied conditions as possible during a succession of lunations, publishing drawings on a large scale of the several regions for the use of future selenographers.

Kempston, Beds: August 20, 1885.

THE MARKINGS ON JUPITER.

In briefly reviewing observations obtained here during the past opposition, it is satisfactory to note the continued interest in the phenomena of this planet; and we have now the gratifying prospect that the red spot, instead of lingering on the point of disappearance, is recovering some of its former prominence, so that the probability is it will again present a conspicuous aspect when Jupiter reappears as a morning star next October.

During the past winter the central portion of the spot became obliterated by a light cloud, though the oval red margin of the spot retained its integrity and could be well observed as it passed the planet's central meridian. Later in the opposition it was noticed, however, that the interior region of the spot was becoming darker, and the surrounding ring much darker, than before. In May and June I carefully examined the spot on several nights with my 10-in. reflector, power 252, and on May 9, under very good definition, saw that a belt in greater S. latitude, and west of the spot, had become connected with it by a ligament tapering down to its southern edge. On this, and several other occasions, the spot was remarked as very distinct, much more so indeed than it had been at any time during the two preceding years. The following observations of its passages across the central meridian have been recorded here since the middle of April last:—

1885.		Red Spot Obs. on C.M.			Spot follows Marth's 1st. Meridian.			Longitude of Spot. (Daily rate 878°·34.)
		h.	m.		h.	m.		°
April	17 ...	9	54	...	0	11·4	...	7·0
	27 ...	8	13	...	2	24·1	...	87·9
May	9 ...	8	8	...	5	0·9	...	183·5
	11 ...	9	52	...	5	33·5	...	203·4
June	9 ...	8	51	...	1	59·7	...	73·0
	26 ...	7	59	...	5	43·5	...	209·5
	28 ...	9	37	...	6	9·7	...	225·5
July	8 ...	8	2	...	8	26·2	...	308·8

Comparing the last two observations of this series with the first one obtained during the present opposition (Sept. 21, 1884, 18h. 28m.) I find the rotation period has been 9h. 55m. 39s·2, which is very nearly the same as that (9h. 55m. 39s·1) derived from my observations during the two preceding apparitions. We may, therefore, rely upon it that during the past three years the motion of the spot has exhibited a consistent velocity, and that the retardation, so well marked between 1879 and 1882, ceased to operate, as the spot became very faint in the latter year. During the recent opposition the whole period included by the observed transits equalled 279d. 15h. 9m., during which 700 rotations were performed, as against 710 in 1883–4, and 674 in 1882–3.

My last observation of the season was obtained on July 8, at 8h. 2m., when the outline of the spot was clearly distinguished notwithstanding the low altitude and small diameter of Jupiter. The air was very clear, and I soon picked up the planet with my comet eye-piece, magnifying about 25, and with a field of 70′. Definition was marvellously good, though the sky was extremely bright with the lingering rays of the sun, and the image looked somewhat faint, though rapidly improving as the sun set. At 8h. 22m. the white spot was splendidly seen as it came to the central meridian and lying to the N.W. of the following end of the red spot. It was very brilliant, and its appearance most striking, as it usually is, from the contrast with the dark material involved with the equatorial margin of the great southern belt. Transits of the white spot have been noted here on the following occasions since the beginning of April :—

1885.	White Spot Obs. on C.M. h. m.	Spot precedes Marth's 1st Meridan. m.	Longitude (Daily rate 878·°34.) °	Appearance.
April 3	10 38	36·7	337·6	Very bright.
6	7 28	38·3	336·6	Bright, train to W.
17	9 7	35·6	338·3	Faint.
19	10 21	32·9	339·7	Very faint.
May 1	7 41	30·6	341·3	
12	9 10	39·4	336·0	Very bright.
14	10 24	37·0	337·4	,, ,,
16	11 32	40·5	335·3	Very very bright.
June 13	8 12	62·7	321·8	,, ,, ,,
July 1	9 13	57·1	325·4	Very bright.
8	8 22	64·2	320·8	Very very bright.

On July 8 the equatorial bright spot followed the red spot by 20 minutes, so that the differences in the velocities of the two

markings (about 8°· per day) would bring them into conjunction on July 10. During the past opposition the white spot has been influenced by some astonishing fluctuations in its rate of motion. The mean period derived from observations between October 4, 1884, and July 8, 1885 (= 675 rotations), is 9h. 50m. 58·2, which is 7 seconds less than that shown for the same object during the two preceding years. This remarkable acceleration of speed appears to have occurred chiefly between November 21, 1884, and January 13, 1885, when the rotation period was only 9h. 49m. 38s·45, or 34 seconds less than the average period between 1882 and 1884. After the middle of January, 1885, the spot seems to have resumed its former speed; but between May 16 and June 13 another great displacement occurred, amounting to some 22°, so that the spot passed the central meridian fully 36 minutes after the computed time.

Some singular variations in the motion of this equatorial spot had been noticed during previous oppositions, but they were of far minor character to those more recently affecting the spot. Of their actual occurrence there can be no question. The only cause of error would be in the fact that different spots were assumed to be identical. If the successive observations were of nearly similar, bright markings, closely preceding each other in longitude, then we could readily understand a supposed great increase of velocity, but no such explanation may be applied to the phenomena under consideration. The white spot to which the observations refer, was far more brilliant than any other on the disc of Jupiter, and comparatively isolated during the period of its increased speed, so that no complications in observing different features could possibly have occurred.

I now pass on to consider other details; and one of the most interesting of these has latterly consisted of several very large bright spots indenting the N. margin of the northern equatorial belt. During the last half of the opposition one of these was situated in the same longitude as the centre of the red spot. I made a number of observations of this object, and of another similar one which I noticed preceding the red spot by about 2h. 52m. on January 27, 1885. These bright spots underlying the N. belt are evidently of different nature to those which are so persistent bordering the S. belt and near the equator, the relative velocity showing a marked discordance. The latter complete a rotation in 5m. 27s. less time than the red spot, while the N. spots differ very slightly indeed in their periods from that object.

The dark reddish spots referred to by Mr. N. E. Green (*Register*, April, 1885, p. 95, and May, p. 116), have also

attracted considerable attention. They are situated in the same latitude as the old red spot, and the chief spot follows the red 1h. 48m. It has been certainly visible since September 25, 1883, when it was described by Mr. Franks as "a faint elliptical spot, devoid of colour, or strength of outline;" but, since that epoch it has developed into a very conspicuous marking. It has displayed precisely the same rotation period as the old red spot, and is projected upon the narrow dark belt which formed on the immediate S. margin of the great S. belt in the autumn of 1881. This narrow belt, contemporary with the outbreak of these reddish spots, has become much more feeble, and on the W. side of the old red spot it is nearly obliterated; thus, the singular hollow in the great S. belt, enclosing the red spot on its N. side, has lost its familiar configuration.

The surface markings of this planet give promise of considerable variety and importance during the ensuing opposition, and, with the transits of the satellites and their shadows, form a series of phenomena of great interest. I subjoin the most favourable times when the red spot and white equatorial spot may be expected to pass the planet's C.M. in October and November next; but with reference to the white spot, the variable motion may originate a considerable error in the predicted times. It should, therefore, be looked for some time before and after that predicted. As to the red spot, its places are capable of far more accurate prediction, as it is certainly not displaced by any such curious and intermittent oscillations as those of the white spot:—

Date. 1885.		Red Spot on C.M.	White Spot on C.M.	♃ rises.	☉ rises.	♃ precedes ☉
		h. m.	h. m.	h. m.	h. m.	h. m.
Oct.	7 ...	18 34	18 34	16 3	18 14	2 11
	12 ...	17 43	...	15 49	18 23	2 34
	14 ...	...	17 49	15 43	18 26	2 43
	16 ...	...	19 0	15 38	18 30	2 52
	17 ...	16 52	...	15 35	18 31	2 56
	19 ...	18 30	...	15 29	18 35	3 6
	21 ...	...	17 3	15 23	18 38	3 15
	23 ...	...	18 14	15 17	18 42	3 25
	24 ...	17 39	...	15 14	18 44	3 30
	25 ...	...	19 25	15 11	18 45	3 34
	26 ...	19 17	...	15 9	18 47	3 38
	28 ...	...	16 18	15 4	18 51	3 47
	29 ...	16 48	...	15 1	18 53	3 52
	30 ...	...	17 29	14 58	18 54	3 56
	31 ...	18 26	...	14 55	18 56	4 1
Nov.	1 ...	...	18 40	14 52	18 58	4 6

Date: 1885.		Red Spot on C.M. h. m.	White Spot on C.M. h. m.	♃ rises. h. m.	☉ rises. h. m.	♃ precedes ☉ h. m.
Nov.	3 ...	15 56	...	14 46	19 1	4 15
	4 ...	...	15 32	14 43	19 3	4 20
	5 ...	17 34	...	14 40	19 5	4 25
	6 ...	...	16 43	14 37	19 7	4 30
	7 ...	19 13	...	14 34	19 9	4 35
	8 ...	...	17 54	14 31	19 10	4 39
	10 ...	16 43	19 5	14 25	19 14	4 49
	12 ...	18 21	...	14 19	19 17	4 58
	13 ...	...	15 57	14 16	19 19	5 3
	14 ...	19 59	...	14 13	19 21	5 8
	15 ...	...	17 8	14 10	19 22	5 12
	17 ...	17 29	18 19	14 4	19 26	5 22
	19 ...	19 7	19 30	13 58	19 29	5 31
	22 ...	16 37	16 21	13 49	19 34	5 45
	24 ...	18 15	17 32	13 43	19 37	5 54

Thus, on the morning of October 8 next, soon after sunrise, the two spots should be seen in nearly coincident longitude. Again, on the mornings of November 20 and 23, they will be very near together. At the former epoch Jupiter will be entering the extreme W. boundary of Virgo, and his position is 3° f. β Virginis and 12° S. of β Leonis.

Bristol: July 17, 1885. W. F. DENNING.

OBSERVATORIES OF EUROPE AND AMERICA.

(Continued from page 190.)

Observatory of Rio de Janeiro. At the end of the last century the Portuguese missionaries determined the longitude and latitude of the city of Rio de Janeiro, but Brazil did not possess an observatory till long after the Declaration of Independence, under the reign of the present Emperor, Don Pedro II. At the west of the immense bay of Rio de Janeiro, the finest in the world, is a detached hill, *Morro do Castello,* quite on the border of the sea, the summit of which is nearly entirely occupied by a vast establishment, formerly built by the Jesuits. It was on the fine terrace of this ancient convent that Don Pedro, in 1845, installed the observatory of Rio de Janeiro. Its first director was M. Soulier de Sauve, who had little experience in astronomy, and who appears to have done very little work. General Antonio Manoel de Mello succeeded him in 1850, but

he did not instil much more activity into the astronomical studies of the place. His only observations seem to have been made with the meridian circle, in order to give the time of noon to the numerous vessels in the roads. Appointed commander-in-chief of the Brazilian cavalry during the war with Paraguay, General de Mello, then seventy years old, soon succumbed to the fatigues of war. His successor was his former aide-de-camp, Cruvello d'Avila, captain of a frigate of the Brazilian navy, who presided over the observatory till 1871, in whose time no more observations were made than before.

The observatory, since 1850, possessed a telescope 4·3 in. aperture, a meridian circle nearly 4 feet in diameter, and an equatorial 6·3-in. aperture, from the workshops of Dollond; but the last two instruments had never been placed in position. The only real work of the observatory had consisted in the publication, pretty regularly, of astronomical ephemerides for the use of the Brazilian navy, and published under the title, *Ephemerides do Imperial Observatorio Astronomico do Rio de Janeiro.* We should, however, mention that since 1851 an important and continuous series of meteorological observations were carried on, and regularly published under the title of *Annales Meteorologicos do Rio de Janeiro.* But this observatory did not really take a place of importance amongst astronomical establishments until after it was placed under the direction of M. Emm. Liais, formerly astronomer at the Paris observatory.

Charged, in 1858, by the Minister of Public Instruction of France with a scientific mission to Brazil, M. Liais arrived at Rio de Janeiro at the time when the Imperial Government was preparing the expedition to observe at Taranagua (about 3° south of Rio) the total eclipse of the sun of the 7th September, 1858. On the invitation of the government he joined the Brazilian commission. The observation of this fine phenomenon enabled M. Liais to prove that the solar corona is due to the atmosphere which surrounds the sun, a consequence which he verified later, at the total eclipse of the 25th April, 1865, which he viewed at the observatory of Rio. Justly appreciated by H.M. the Emperor of Brazil, M. Liais returned to France only with the intention of returning very soon to Brazil, and remaining there definitely. From 1860 to 1870 he went over that vast empire in every sense, studying its manners and productions, resuming and superintending the work of revising the chart of the coasts of Brazil, whilst not neglecting his favourite astronomical pursuits. At his temporary observatory of Olinda he discovered, on the 26th February, 1860, in the constellation Dorado, a double comet (Comet I., 1860) which he continued to observe till the 13th March. He

also observed at Rio the great comet of 1860 (Comet III., 1860), which suddenly appeared on the horizon in France and the middle of Europe about the 22nd of June in that year. The following year he examined with great care the fine comet (Comet II., 1861) discovered at Sydney on the 26th of May, the tail of which, on June 11th, at Rio de Janeiro, extended more than 40 degrees in length. M. Liais thought that he could infer from the totality of his observations that on the morning of June 29th the earth had traversed rather obliquely the tail of this comet, the breadth of which was then above a million of leagues.

On the death of the commandant, Aguilar, M. Liais was charged by H.M. the Emperor of Brazil to take the direction of the observatory of Rio de Janeiro. Almost every thing had to be done to place the establishment in the rank which the liberality of the emperor admitted of its attaining, as well as to give its work a scientific and really useful direction. It was necessary, in the first place, to place the old instruments in position, and complete the apparatus of the observatory; and in the next to provide for the recruiting of the staff required by every observatory of importance. As to the instruments: in October, 1875, they were as follows:—1. A meridian telescope, by Dollond, 4-inch aperture, with micrometer and collimators. 2. A meridian circle, by Dollond, nearly 4 feet in diameter, with collimators; a bath of quicksilver, and nadir eye-piece; a small movable telescope for zenith collimation, and a collimator apparatus to verify the rotation of the optical axis in one plane, and to measure any deviations, so as to correct the observations. 3. An instrument in the prime vertical, 4-inch aperture, furnished with a collimator in the direction of its axis, which can be used directly on the meridian-circle, or on the fixed mark,* or on the collimator with long focus of the same circle. This arrangement allows of the determination of the meridian of the two instruments by combining the observations with each, a very important object in the equatorial stations of the southern hemisphere, where there is almost always a want of circumpolar stars that can be observed at their two passages on the same day. 4. An azimuthal instrument of great size, constructed at Rio itself, in the workshops of José Maria dos Reis, under the direction of M. Liais, and similar to the one which that astronomer had set up in 1860 in his temporary observatory of Olinda. It would not be easy to describe this fine instrument without the accompanying print of it given in MM. André and Angot's work.

* The telescope is then placed vertically, and the central cube of the instrument is opened.

Suffice it to say that the telescope is 4-feet focal length, that there are two small collimating telescopes, and a small repeating theodolite. A permanent azimuthal mark is set up, and observations are made chronographically. The above four instruments are not absolutely independent of each other; when it is desired, they can be used conjointly, one with others, and thus afford each other mutual support. Thus the collimation of all four can be combined, so that the meridian enters only once as an unknown quantity in the equations furnished by observations made with the four instruments. There result various notable methods, both for the determination of the absolute latitude of the observatory, and obtaining absolute declinations of the stars without having to fear the influence of refraction and flexure.*

To these meridian instruments, which, as is seen, form a complete whole, must be added, 5: A large equatorial, by Dollond, the object-glass of which, worked by MM. Henry, of Paris, is 9·8-in. aperture. 6. A second equatorial of 6-in. aperture, under a rolling hut, which is quite uncovered for observations. It is especially designed for looking for comets, and solar photography. It is furnished with a complete position micrometer. 7. A small equatorial, 3-in. aperture, with a large field for a rapid sweeping of the southern heavens in searching for comets. 8. Lastly, a zenith telescope, by Porro, with his ingenious system of micrometers. All the instruments are connected with the same chronograph, on which all observations are inscribed. In a short time the observatory will be enriched with two other special instruments, which are in course of construction. One is a gigantic altazimuth, the telescope of which will be 14·6-in. aperture. It will be furnished with a collimator in the axis, and a complete set of collimators. This fine instrument is being constructed in the workshops of the observatory, the principal pieces having been cast and prepared in the arsenal of war. The second instrument is the *cœlestat,* constructed at Paris by M. Secretan. This apparatus, analogous to the siderostat, is so arranged as to send in one fixed direction rays proceeding from any point in the heavens, which is impossible with the siderostat. Two mirrors, in place of one, are employed, and such a combination of them is adopted that the luminous rays never make with the normal at each of them a greater angle than 30 degrees; by which means the influence of the defects incidental to the mirrors is considerably diminished. The clockwork movement which carries the mirrors of the cœlestat is, like that of Foucault, of constant

* A recent determination by M. Manuel Pereira Reis of the latitude of the observatory is 20° 54′ 23·7″, which, according to M. Liais, cannot be more than 1″ in error.

velocity; but by means of a simple change of weight it is possible to vary that velocity so as to adjust it to any star, the sun, moon, and planets. The instruments employed with the cœlestat are two reflectors, 15·7-in. aperture, one in black glass for observing the sun, the other in silvered glass for stars and planets. The cœlestat is, above all, intended for the researches of physical astronomy, and spectroscopic work in particular. The observatory possesses, in addition, a complete set of magnetic self-registering instruments, and a self-registering apparatus for meteorology, by Dollond, of London.

Although the installation is hardly finished, astronomical work has already begun. In July and August, 1875, an important series of observations of Mars in opposition was made by the method which M. Liais had already applied in 1860 to the same phenomenon, in order to find the value of the solar parallax. M. Liais has been led to the value 8″·78. A complete and systematic study of the instruments has been actively pursued. The catalogue of the fundamental stars of the southern hemisphere is begun; consecutive observations of the sun's declination were made in December, 1876 (the sun was then near the zenith), for the determination of the obliquity of the ecliptic; also a series of observations with the altazimuth, with a view of obtaining the value of the constant of aberration. The observatory is besides charged with the direction of the operations relative to the formation of the trigonometrical chart of the empire, the measure of meridians and parallels, and the determination of the osculating ellipsoid of the surface of the earth in the region of Brazil. One of the astronomers, M. Manoel Pereira Reis, has already determined the difference of longitude and latitude between the Observatory of Rio and Barra do Pirahy. He has made use in his observations of some of the improvements devised by M. Liais, and that one in particular which admits of the transformation of a transit instrument into a zenith telescope, the first idea of which is however due to M. Faye. If to all this we add so many and useful researches in physical astronomy, we see that the programme of the observatory of Rio de Janeiro is most extensive, and well conceived. It is but fair to say that H. M. Don Pedro, to whom the reorganisation of the establishment is due, takes great interest in the work done there, and secures to it a considerable endowment.

Observatory of Lima (Peru). In 1866, the Peruvian Government charged M. Cabello to found an observatory at Lima. After a long sojourn in Europe M. Cabello caused to be constructed at Paris, in the workshops of M. Eichens, the instruments with which the future establishment was to be furnished.

These were, a meridian circle, with telescope 7-in. clear aperture; a transit instrument of the same power; a comet seeker of the same aperture; and an equatorial of 10·5-in. aperture, similar to those which had been constructed for the Observatory of Marseilles. MM. André and Angot give some details, and a representation of the meridian circle, the first made on that model in France. It is read by six microscopes; the micrometer, which has 16 vertical, and 3 pairs of horizontal wires, is illuminated with either dark wires on an illumined field, or bright wires on a dark field.

Unhappily, revolutions, so frequent in that country, have prevented the giving effect to the original purpose. The instruments are actually still in their boxes, and there is no hope of the early erection of the observatory itself. This is much to be regretted; and we should be glad if the account we have given of the efforts made in the neighbouring countries, and the results which have been already achieved there, might contribute in some measure to the turning to account of the fine instruments which are possessed by the Peruvian Government.

More than half of this volume of MM. André and Rayet's account of the observations of the world is occupied with a detailed description of the meteorological institutions of the United States, beginning with the Smithsonian Institute, which was created by James Smithson, a descendant of one of the Dukes of Northumberland, who died in 1828, and left the interest of his fortune to his nephew, on condition that he should come to the United States of America "to found, under the name of the Smithsonian Institute, an establishment destined to promote the development of science amongst men." Congress, by a decree of July 1, 1836, accepted this legacy, which then amounted to 3,250,000 francs (650,000 dollars), of which the interest only is used, and spent over 2,250,000 francs in building the palace of the institution, the first stone of which was laid on May 1, 1847, but which was not finished till 1856. Professor Joseph Henry was Secretary-Director till his death in 1878. To his exertions the institute owes much of its development. It has pursued two principal objects. Firstly, to facilitate the production of original work in science by grants of money to learned men, and bringing out their publications in the *Smithsonian Contributions to Science, The Miscellanies,* and the *Annual Reports* to Congress. Secondly, the study of meteorology, and the climatology of America in particular. Since 1874 the Smithsonian Institution has been obliged somewhat to contract the field of its operations. The failure of a bank, where its capital was deposited, diminished its resources, and the formation of the

Signal Service in great measure superseded its meteorological work. All observations are now sent to the Signal Service, and the Smithsonian Institute confines itself to the discussion of the data furnished by the Signal Service, and deducing the general laws of the climate of America. These two great institutions thus afford each other mutual support. A very full account follows of the Signal Service, its central administration, its staff, organisation of stations, organisation of service, Central Bureau of Washington, special missions, statistics, publications, results obtained. The volume from which we have extracted the above notices contains illustrations of observations and instruments, and a chart, meteorological and hypsometrical, of the United States.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER

ALPETRAGIUS d.

SIR,—In his Selenographical Notes in the July Number of the *Register*, Mr. Elger has called attention to a formation, Alpetragius *d*, which seems deserving of further study.

Some few months after the appearance of Professor Schmidt's letter to Mr. Birt in *The Student*, vol. ii., p. 48, I had one or two opportunities of observing the region with my $7\frac{1}{3}$-in. refractor, namely, on November 5, and December 23, 1868, and on February 20, 1869. On the second of these dates I detected on *d*, which itself presented the appearance of a brilliant white spot, a small craterlet a little to the north of its centre, and about half the diameter of the small crater figured by Beer and Mädler on the south side of it. This craterlet was again very distinctly seen on February 20, 1869, on which occasion a rough sketch of the region was made at the telescope. On this date I made the following remark in my observing book: "The white spot is, I think, larger than that of Linné, and the craterlet perhaps rather larger than that of Linné also."

I was hoping that I might be able to re-examine the region before making any communication to you on the subject; but as I failed, owing to bad definition, to get a satisfactory view on July

20 and 24, and as I have little hope of being able to observe it during either this or the next lunations, I think it best to send you this account of my old observations without further delay.

I am, sir, yours faithfully,

GEORGE KNOTT.

Knowles Lodge, Cuckfield: August 18, 1885.

METEORS.

Dear Sir,—For three evenings we have had an unusual display of meteors, nearly every one of which were Perseids. They were very large and bright, and nearly every one followed the galaxy, and gradually approached the earth. Last night, however, was the grandest display. The air was clear, and the night very dark. Many of the meteors left trails of a sulphurous hue forty degrees in length; these appeared bright for from five to thirty seconds, and in three instances their trails wholly disappeared, and again became luminous. This was repeated two or three times in both instances. This was no illusion of the eye, for I am acquainted with optics. I account for it by the matter in the trail being phosphorescent and coming in contact with waves of moisture, which were put in motion by the meteors disturbing the equilibrium of the atmosphere; for I noticed that these trails were of a lower altitude than most of the others. I noticed one large meteor which seemed to have a rotary motion, as a shell fired from a rifled gun. Has the same ever been observed of other meteoric showers?

Yours truly,

R. GABRIEL.

Milford Centre, Ohio, U.S.A.: August 11, 1885.

REVIEWS.

Sur l'Agrandissement apparent des Constellations du Soleil et de la Lune à l'Horizon. Par M. Paul Stroobant. Bruxelles: F. Hayez, 1835, pp. 15.

Observations and the results of some ingenious experiments lead the author to conclude that "what is constant is that the enlargement (apparent) of the moon on the horizon relates to two very distinct causes, the first essentially subjective, which is that every object situated at the zenith appears only 0·8 of its size on the horizon of the observer, and the second, owing to the brightness of the moon after it is some degrees high, becoming strong enough to produce a contraction of the pupil of the eye that reduces its size to 0·7 of that which it seemed to be when close to the horizon. For the constellations the first cause alone is in question. For the sun's, the second cause is only very sensibly added to the first when the air is very misty. This explains why the sun's apparent size is more variable than the moon's on the horizon.

DUN ECHT CIRCULAR, No. 96.

A telegram from Professor Krueger announces the discovery of Tuttle's comet by M. Perrotin, at Nice, the observed place being :—

Greenwich M. T.	A. R.	Dec.
1885, Aug. 9·6124	7h. 23m. 43s·1.	N. 28° 1′ 24″.

The following places are from an ephemeris by Herr Rahts in the *Astronomische Nachrichten*, No. 2674 :—

12h. Berlin.	R. A. h. m. s.	Decl. ° ′	Brightness.
Aug. 14	7 40 4	+24 49·1	0·46
„ 18	7 53 6	22 5·0	0·49
„ 22	8 5 58	19 12·1	0·52
„ 26	8 18 41	+16 10·8	0·54

The comet's brightness when first seen in 1871, has been taken as unity. It will remain very faint during the whole of this return.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1885, Aug. 11.

THE PLANETS FOR SEPTEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	10 46 1 / 10 48 44	N. 3 26 / 3 58	10″·6 / 10″·4	0 2·8 / 23 15·6
	9th	10 25 8	N. 8 10	9″·0	23 6·6
	17th	10 38 59	N. 9 15½	7″·2	22 48·4
	25th	11 20 26	N. 6 11½	5″·8	22 58·8
Venus ...	1st	12 41 3	S. 3 49	12″·3	1 57·5
	9th	13 25 8	S. 8 53	12″·9	2 1·1
	17th	13 51 59	S. 11 47½	13″·2	2 5·3
	25th	14 28 29	S. 15 25½	13″·8	2 10·3
Saturn ...	1st	6 27 29	N.22 24	15″·8	19 41·0
	9th	6 30 4	N.22 22½	16″·0	19 12·1
	17th	6 32 15	N.22 21	16″·2	18 42·8
	25th	6 33 59	N.22 19½	16″·3	18 13·1
Neptune ...	30th	3 33 37	N.17 23½		14 53·6

Mercury rises close to the sun, at the beginning of the month, and afterwards earlier each night, the interval decreasing again towards the end of the month.

Venus sets about an hour after the sun throughout the month.

Saturn rises half an hour before midnight on the 1st, the interval increasing.

ASTRONOMICAL OCCURRENCES FOR SEPTEMBER, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
Tues	1	17 14 10 52 10 51 11 1 11 32 11 9 11 21 12 1 13 4	☾ Moon's Last Quarter Occultation reappearance of θ^2 Tauri (4½) Occultation reappearance of θ^1 Tauri (4½) Occultation of B.A.C. 1391 (5) Reappearance of ditto Near approach of 81 Tauri (5½) Occultation of 85 Tauri (6) Reappearance of ditto Near approach of α Tauri (1)			— 12 13·8
Wed	2	11 20 12 13 12 48 6	Occultation reappearance of 117 Tauri (6) Occultation of B.A.C. 1728 (6) Reappearance of ditto Inferior conjunction of Mercury and Sun	Jupiter's		12 9·9
Thur	3	16 57 15	Near approach of 26 Geminorum (5½) Conjunction of Moon and Saturn 4° 17′ N.	Satellites		12 5·9
Fri	4	12 58 19	Near approach of 68 Geminorum (5½) Conjunction of Moon and Mars 5° 33′ N.	invisible		12 2·0
Sat	5		Saturn's Ring: Major axis=39″·99 Minor axis=17″·34	through		11 58·1
Sun	6		Sidereal Time at Mean Noon 11h. 2m. 57·87s.	his		11 54·1
Mon	7	16	Conjunction of Moon and Mercury 0° 27′ S. Sun's Meridian Passage 2m. 11·80s. before Mean Noon	nearness		11 50·9
Tues	8	8 43 10 10	● New Moon Eclipse of the Sun invisible at Greenwich Conjunction of Moon and Jupiter 1° 57′ N. Conjunction of Jupiter and Sun	to		11 46·3
Wed	9		Sun Meridian Passage 2m. 52·58s. before Mean Noon	the		11 42·3
Thur	10	22	Conjunction of Moon and Venus 2° 27′ S.	Sun.		11 38·4
Fri	11					11 34·5
Sat	12					11 30·5
Sun	13					11 26·6
Mon	14					11 22·7
Tues	15	18 14	☽ Moon's First Quarter Illuminated portion of disc of Venus=0·806 Illuminated portion of disc of Mars=0·928			11 18·7
Wed	16		Sidereal Time at Mean Noon 11h. 42m. 23·38s.			11 14·8

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
Thur	17		Sun's Meridian Passage 5m. 40·27s. before Mean Noon			— 11 10·9
Fri	18					11 6·9
Sat	19	8 4 9 23 9 33 10 41	Occultation of 13 Capricorni (6) Reappearance of ditto Occultation of 14 Capricorni (5) Reappearance of ditto			11 3·0
Sun	20	6 47 7 55	Occultation of 18 Aquarii (6) Reappearance of ditto			10 59·1
Mon	21	10 8 11 22	Occultation of B.A.C. 7774 (6) Reappearance of ditto	Jupiter's		10 55·1
Tues	22			Satellites		10 51·2
Wed	23	19 54 17 12	○ Full Moon Occultation of B.A.C. 8365 (6½) Eclipse of the Moon invisible at Greenwich	invisible		10 47·3
Thur	24			through		10 43·3
Fri	25	8 12 9 9 19	Occultation of μ Piscium Reappearance of ditto Conjunction of Sun and Uranus Saturn's Ring: Major axis=41″·39 Minor axis=17″·81	his		10 39·4
Sat	26	9 19 10 3 21	Occultation of B.A.C. 741 (6½) Reappearance of ditto Conjunction of Jupiter and Mercury 0° 52′ N.	nearness		10 35·5
Sun	27			to		10 31·5
Mon	28	9 19 10 3 11 46 12 43 16 41 17 41 16 48 17 26 17 41 14 51	Occultation of 48 Tauri (6) Reappearance of ditto Occultation of ν Tauri (4) Reappearance of ditto Occultation of 75 Tauri (6) Reappearance of ditto Occultation of θ¹ Tauri (4½) Reappearance of ditto Occultation of B.A.C. 1391 (5) Reappearance of ditto	the Sun.		10 27·6
Tues	29	15 2 16 17 17 15 18 5	Occultation of 111 Tauri (5½) Reappearance of ditto Occultation of 117 Tauri (6) Reappearance of ditto			10 23·7
Wed	30	23 29 14 23	☾ Moon's Last Quarter Saturn at quadrature with the Sun Conjunction of Moon and Saturn 4° 15′ N.			10 19·8

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
Sept. 1	— 2 5 E.	Sept. 11	+55 52M.	Sept. 21	—66 8M.
2	14 17	12	43 40	22	78 20
3	26 30	13	31 28	23	+89 29 E.
4	38 42	14	19 16	24	77 16
5	50 54	15	7 3	25	65 5
6	63 7	16	— 5 8	26	52 53
7	75 19	17	17 21	27	40 42
8	87 32	18	29 32	28	28 30
9	+80 17M.	19	41 44	29	16 19
10	68 4	20	53 56	30	4 7

Moon nearest the Earth, Sept. 6, 2h.
„ farthest from „ „ 17, 22h.

Books received :—Cincinnati University. Observation of Comets. 1883. By H. C. Wilson, A.M. and J. G. Porter, A.M. Cincinnati. 1885. —Revue Mensuelle d'Astronomie Populaire. By M. Flammarion.—Astronomische Nachrichten.—Report of the Natal Observatory. 1884.—Results of the Astronomical Observations made at the Melbourne Observatory. 1876 to 1880. By R. L. J. Ellevy. Melbourne. 1884.—Possibility of Errors in Scientific Researches due to Thought transference. By E. C. Pickering.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To June, 1885.	August, 1885.	To Dec., 1885.
Howlett, Rev. F.	Worthington, A. M.	Grove, S.
Tupman, Colonel.		King, T. B.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor 11, Angel Court, Throgmorton Street, London, E.C.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must inclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to JOHN C. JACKSON, Chief Office, London.

The Astronomical Register.

No. 274. OCTOBER. 1885.

EXPEDITION TO OCEANIA FOR THE OBSERVATION OF THE TOTAL ECLIPSE OF THE SUN OF MAY 6, 1883.*

We give some extracts from M. Janssen's interesting narrative of this expedition. He says: "The expedition with which we had been charged by the Government, the Academy, and the Bureau des Longitudes, was designed, as is known, for the observation of the great total eclipse of the 6th May last, and to take advantage of the rare duration of this phenomenon for endeavouring to solve certain questions about the constitution of the sun, and the existence of planets called intra-mercurial. The place of observation had been fixed for Caroline Island, in 152° 20′ west longitude and 10° south latitude—that is to say, nearly on the meridian of our fine island of Tahiti, but about 200 leagues further north. This station, not absolutely on the line of total phase, was yet very near it. It was considered the one least unfavourable for the observation of a phenomenon which visited only the maritime parts of Oceania.

"Our party had been joined by MM. Tacchini, the able director of the Observatory of Rome, and Palisa of the Observatory of Vienna, the discoverer of so many new minor planets. The French part of the expedition consisted, besides its chief, of M. Trouvelot, astronomer attached to the Observatory of Meudon, M. Pasteur, photographer, and an assistant. Circumstances

* Report to the *Bureau des Longitudes*, read at the Séance of Sept. 5, 1883, by M. Janssen (from the *Annuaire*, published by the *Bureau des Longitudes*, 1884).

beyond our control had left us but very little time for preparations for this great expedition; yet the study of the questions before us necessitated the employment of new instruments of great size; in the use of which it was very desirable that we should be practised and familiar beforehand. Such practice was not possible; and we had to make the greatest efforts in order to reach our station early enough to admit of a sufficient trial of our huge apparatus. This consisted for the French party, independently of the large instruments of MM. Tacchini and Palisa, of equatoreals of 6 and 8 inches, intended for the ocular search of intra-mercurial planets, a large parallactic stand of steel and cast-iron carrying photographic apparatus for taking photographs of the corona and the regions around the sun; reflecting-telescopes of 20 and 16 inches, meridian instruments, tents, &c. It was the equipment of a great second-class observatory, which was to be transported to a distance of above 4,000 leagues.

"We left Saint-Nazaire on the 6th March, in the steam-packet *Nazaire*, of the Transatlantic Company; and on the 27th we landed at Colon. We had every reason to be satisfied with the treatment of the mission by the Company. At Colon, I found M. Charles de Lesseps, whom matters about the canal had brought to the isthmus. He received us with great courtesy, and greatly facilitated the transport of our luggage from Colon to Panama. At Panama we found the ship of war *l'Eclaireur*, which M. the Minister of Marine had kindly placed at our disposal, and which had received orders to leave Callao, take us up at Panama, and bring us straight to Caroline. It was then near the end of March, and we had about 4,300 miles to get over to reach our station. The situation was almost critical. I induced the commander to take in an extra supply of coal, which, together with our great quantity of luggage, was placed on the bridge. It was this supplementary provision, though still in my opinion inadequate, which was the saving, so to speak, of the situation; for we found the trade-wind very feeble in the region where it was expected to be fresh and constant. The aid afforded by the wind during this long voyage was in fact very small, and the passage had to be made almost entirely by steam. We first made for the Marquesas, where we coaled, thus accelerating somewhat our arrival at Caroline. Finally, on the 22nd, in the evening, we were in sight of the island which I was so anxious to reach. The American vessel *The Hartfort* was in the offing. She had just put ashore the American mission, and was preparing to leave for Tahiti. An officer came to visit us, who acquainted us with the difficulties of the disembarkation. Next morning we

commenced this operation. I first landed, accompanied by MM. Tacchini, Palisa, and Trouvelot, to reconnoitre the place, to see the American astronomers, and decide upon our station for observing.

"Caroline is a low island, the part above water entirely formed of coral. It consists of a number of islets disposed in a circular form, and united together by coral reefs just below the surface of the water, on which the sea breaks continually. On the side where we landed there is a very narrow opening in the reef, barely sufficient for the passage of a boat. This channel had been described to us by the American officer, and by it we penetrated into the lagoon or interior sea. The American Astronomical Commission received us with great cordiality. These gentlemen, although they had left Lima a fortnight before us, had only arrived two days before. Caroline Island contains some rather important deposits of phosphate of lime, produced by the guano washed by the copious rains of those regions. This phosphate is worked by a mercantile house (Messrs. Houlder Brothers, of London), which sends workmen there from time to time, and for its operations has caused to be constructed two large wooden cottages. These were, at our arrival, already occupied by the American astronomers, but their chief, M. Holden, kindly offered me one of them. On that desert island, where storms and deluges of rain are so frequent, it will easily be imagined how welcome such a resource was to us; it is also only fair to thank the proprietors for it.

"This important point settled, we fixed upon the spot for our observations, and returned on board the vessel for the operation of disembarking. The boxes were tackled and lowered into a boat. When this was full, it was directed to the entrance of the channel, and by a rapid manœuvre brought through into the calm waters of the interior lagoon, where, however, it was soon brought up for want of depth. The boxes were then transferred to a small flat-bottomed boat, drawing but a few inches of water, which, pushed by the sailors, who walked in the water, brought the load very near the bank. They then took the boxes on their shoulders and brought them to the place that we had chosen. Our heavy boxes required as many as fifteen or eighteen porters. The most difficult part of the business was traversing the channel in the coral reef. The sea broke there with fury, and the reef, hidden beneath the waters, made it difficult to perceive the exact position of the entrance. It was needful to seize the moment when the lowering flood disclosed the situation of the opening, and take advantage of the succeeding wave to penetrate at once through the passage to the entrance of the lagoon. In spite of

the cleverness of our gallant sailors, several boats were broken in these difficult operations, and some boxes were wet. These were immediately set apart and opened to guard against evil consequences. The work occupied two days; it was ended on the evening of the 24th. Before leaving, the *Eclaireur*, according to the directions of the Minister, left, at my request, a party of seventeen men, including steersmen, workmen, and sailors. The following morning the sites were marked out, the tents pitched, and the instruments taken out of their boxes. It is necessary now to give a brief description of our apparatus and the plan of our observations.

"For the search for intra-mercurial planets, M. Palisa had a 6-inch telescope of short focal distance and large field, equatorially mounted, and very well suited for the purpose. For the same object, M. Trouvelot had two telescopes at his disposal, one of 3-inch ap. with large field, a reticule, and inner circle of position; and another of 6-inch, with a high magnifying power. The 3-inch telescope, constituting a searcher, and with a field of about $4^{\circ}\cdot5$, was to explore the circumsolar regions, the cross wires allowing of a position being obtained; the inner circle of position, the long divisions of which were engraved on a glass circle, was for the orientation of the details of the corona for the drawing to be made by M. Trouvelot. As to the 6-inch telescope, also furnished with a reticule, it was to test whether a star suspected to be a planet had really a diameter, whilst the reticule enabled its precise place to be found. These telescopes were parallactically mounted; one of the stands had been employed for the last aransit of Venus. To make the noting of a position more rapid, tnd to dispense with readings, which would have occasioned the loss of such precious time, I had made M. Gautier fit to the circles of right ascension and declination some microscopic tracers, each of which, placed under the hand of an assistant, enabled, when the observer ordered, a fine line to be traced across the divided circle and its vernier, so that it was possible, by means of this accurate point of reference, to replace at a subsequent period the instrument in the position of observation and take the requisite readings at leisure. I should add that, agreeably to my proposal, MM. Palisa and Trouvelot divided the work between them, and agreed to explore each one side of the sun. As is known, the great difficulty in these searches for planets during eclipses lies in the brief space of time that is available; and it is therefore of the utmost importance to restrict as much as possible the field to be examined by an observer.

"Such were the arrangements made for the search for intra-mercurial planets by eye-observations; but we added a new

element, photography. M. Gautier, under my direction, had prepared for us a parallactic stand having an hour axis 6·5 feet in length, which carried a large and strong platform, on which was fixed the following photographic apparatus:—A large camera with an objective of 8 inches, by Darlot, embracing a field of 20° by 25°, designed for photographing the corona and circumsolar regions with reference to the stars which those regions might offer to view; a second camera with objective of 6-inches, by Darlot, embracing a field of 26° by 35°; a very perfect apparatus by Steinheil for the examination of the corona. A second parallactic mounting carried cameras with objectives of 4 inches of great light-grasping power, designed, with very sensitive plates, to determine the limits of the corona during an exposure embracing the whole period of total darkness.

"For spectrum analysis I had brought two reflectors: one nearly 20 inches aperture of very short focus, furnished with a direct-vision spectroscope of ten prisms, the slit of which could take different angular positions, and could be rapidly opened or closed at the pleasure of the observer; an excellent searcher, furnished with a reticule, was placed near the spectroscope at a distance from it of the visual axes, so that whilst one eye was directed with the searcher on a point of the corona, the other could obtain the spectroscopic analysis of that point. I have before described this very convenient arrangement, employed by me since 1871. This reflector carried, besides, a polariscopic biquartz telescope, by M. Prazmowski, and another by the same optician which gives the rings of Respighi. The whole instrument was mounted on a parallactic stand. Fearing much the maritime climate of Caroline, I had associated with the 20-inch reflector another of nearly 16 inches, carrying the same apparatus; the mirror of this reflector remained in its box, and was not to be uncovered except in the event of the other one becoming seriously affected by the atmosphere of our station. Fortunately I had no occasion to have recourse to this second instrument. By minute precautions I was able, notwithstanding the storms and the humidity of the climate, to preserve my mirror without the slightest damage. We had in addition several other instruments, and a transit instrument, of which we made no use, M. Palisa, in order to relieve us, having kindly taken upon himself the determination of the time.

"Our installation was greatly hindered by a succession of storms during our abode at Caroline. Our tents were thrown down or torn, and our instruments flooded. We had a constant struggle to keep our equipment in a condition for use. The mirror of my telescope had to be taken out every evening, brought to

our dwelling, and placed in air kept dry by a coal-fire. In one of these storms we measured a fall of 2·3 inches of rain. In spite, however, of these hindrances, our preparations rapidly progressed. We had several days running rehearsals to define well the part of each person, and on the day of the eclipse we were ready. In order to establish each one's rights, it had been agreed, at my request, that as soon as the eclipse had been observed each observer should draw up a succinct report of his observations, and that these reports should be read in presence of all, and signed by each of us as an authentication of the reading. But the weather did not seem very favourable, as on the morning itself of the 6th we had a storm, and though the eclipse was observed it was only in a clear interval, when the sky indeed was very pure and transparent, but only shortly before and after totality."

We have seen how admirably M. Janssen prepared everything for the observation of the grand and important phenomena. We now make some extracts from the brief report drawn up by him immediately after the observations. These were partly optical and partly photographical. The former were principally for ascertaining whether the coronal spectrum had a continuous background with bright lines, or whether the Fraunhofer lines generally were there. This was chiefly in view of the question of cosmical matter outside of the sun. "Already," M. Janssen says, "in 1871 I had announced that besides the lines of hydrogen, I had found the line D and several more in the spectrum of the corona. I was able now to ascertain that the background of this spectrum is the complete spectrum of Fraunhofer. The principal lines, notably D, *b*, E, &c., were so pronounced as to leave no possible doubt. I could recognize perhaps a hundred lines." M. Janssen thinks that this greatly advances the question of cosmic matter outside of the sun. He continues, "This was especially the case in the lower or brighter part of the corona, but not to an equal extent at the same distance from the moon's limb. Polarization was very decided, and with already known features. In regard to stars and heavenly bodies in the circumsolar region, the photographs taken will demand a minute scrutiny; but the great power of several of the objectives employed and the long exposure enabled it to be proved that the corona has a far greater extent than is shown by optical examination, whether by the naked eye or in my telescope. Several of our large photographs of the corona are of great clearness, and reveal several important details of structure which will have to be discussed. The corona was entirely fixed in its form during the whole time of total eclipse." (Dated Caroline Island, 6th May, 1883.)

From the report of M. P. Tacchini to M. Janssen, chief of the Mission, we learn that he observed with his Dembowski equatorial and a diffraction spectroscope the prominences and chromosphere several successive days before the 6th, the day preceding which he prepared a ruled circle on which he drew them. After delineating them on the morning of the eclipse, he arranged his diffraction spectroscope and saw the chromosphere invaded by the moon, and noted the commencement of the eclipse. After other observations in the intervals of cloud, he noted the second contact with the finder of Cooke's 6-inch telescope. Shortly before this the fine and very close filaments of the corona, the red stratum, and the large prominence began to be visible. M. Tacchini proceeded to examine the prominence with the spectroscope; he says: "It was rather vivid, with several lines at the bottom, continuous and bright in the upper part, then continuous and faint, almost white in the corona, with the green line. In the spectrum of the great plume (*panache*), which was faint and almost continuous, and only to be seen with a wide slit, I observed two bands which appeared to resemble those I had often seen in the spectra of comets, that is, in the central and less refrangible part. The small dispersion, and the form of the spectroscope with its eye-piece very close to that of the finder, and adjusted the day before, greatly facilitated the observation. My design was general, and the measuring of the lines was not contemplated. When the assistant (helmsman) had called out *two minutes,* I quitted the 6-in. telescope and observed with the greatest care the polarization around the moon, on the corona, and the three large plumes (*panaches*). Outside of these plumes there was no polarization, or, at any rate, it was very difficult to perceive. The phenomenon is exactly as described by Blaserna. This observation took up much less time than I had reckoned upon, and the assistant had not yet called out four, when I was already at the 6-in. telescope.

"I quickly removed the small spectroscope and put in an eyepiece of 60 power. What a spectacle was then presented by the structure of the corona and certain details near the limb! I took many notes, and by reason of a fortunate combination of the line of diurnal motion with the plumes (*panaches*), I was enabled to make a very faithful sketch. There were two regions of high jets (gerbes), fine and silvery, which might even be called white prominences; a new question for physicists to resolve—how to find a way of seeing them. Their character raised some questions in my mind about the comet which I saw in Egypt last year." In conclusion, M. Tacchini says, "I used the lamp for reading the chronometer at the second contact, but at the same

time I may say that I was struck with the small degree of darkness in comparison with the eclipse of May, 1882, which lasted only 1m. 14s. I estimate the darkness here as only double that in Egypt, which certainly is equivalent to saying that the corona was more brilliant, and in effect the plumes (*panaches*) were quite different, that is to say, more grand, in Caroline. The wind annoyed me a little at the observation of first contact, but, nevertheless, I think it a good observation. My steersman sailor, Théophile Charrier, rendered excellent service."

(*To be continued.*)

SELENOGRAPHICAL NOTES, OCTOBER, 1885.

By Thos. Gwyn Elger, F.R.A.S.

Appearances observed in connection with certain Lunar Clefts. A number of very remarkable observations emanating from Mons. E. L. Trouvelot, of Meudon, appear in *Sirius* for July, which demand more than a passing reference, inasmuch as they relate to some features not, so far as I am aware, previously recorded, and to others that have been only incidentally noticed in a few cases. When observing Eudoxus, on February 20th, 1877, with a refractor of about 6½-in. aperture, the terminator passing through Aristillus and Alphonsus, a narrow thread of light of uniform breadth was remarked by M. Trouvelot crossing the southern part of the interior, and extending from wall to wall. On the west, however, its extremity did not quite touch the border, which at this place shows a semi-circular notch or indentation, but was separated from it by a slight interval; while on the east it was traced quite up to the wall, where it was skirted by a kind of fracture or sinuous ravine, which, traversing the margin, extended for some distance beyond, towards the south-east. The western portion of the thin bright line was bordered on either side by shadow, but as it approached the eastern wall the shadow on the south flank gradually died out, leaving a fringe of shade on the north side only. This light stripe, which appeared more brilliant than the neighbouring sun-illumined surface, impressed M. Trouvelot with the idea that the interior of Eudoxus at this place was crossed by a lofty narrow wall, which stood up above (*surgissait*) the associated shadows at the time of observation. On December 31st, 1878, the terminator still lying near Aristillus, there were no signs of

the wall, but in its place something resembling a fracture (Query, a cleft). On May 4th, 1881, Eudoxus being close to the terminator, a bright spot, slightly elongated from north to south, was noticed, exactly at the place where the thread of light observed in 1877 terminated, viz., opposite the notch in the western wall. This object was probably a portion of the craterlet that the notch apparently represents. On April 23rd, 1885, the E. long. of the morning terminator being about 21°, the floor of the ring-plain, then in sunlight, was carefully examined without any traces of the wall being perceived, though on the west rocks and *débris*, arranged in a straight line, were seen on the site of the luminous filament remarked in 1877. In addition to the phenomenon noted in Eudoxus, M. Trouvelot adduces other examples of walls as long and as narrow, which, as in that case, are found either on the course of certain clefts or forming more or less extended prolongations of them. Among these he mentions the prominent cleft commencing north of Burg (marked η in *Neison's Map V.*), and running in a south-easterly direction to a massive mountain (B), where all traces of it seem for a time to be obliterated by the brilliant light emanating from a craterlet on the summit. But it is continued beyond in a straight line, turning up again on the opposite side of the mountain. It next crosses a branch of the Lacus Somniorum, and, still maintaining a south-easterly course, traverses the rough undulating region west of Alexander, and, crossing the Caucasus, finally terminates in the Mare Serenitatis on the north of a minute isolated crater in the vicinity of this range.* M. Trouvelot states that this extended line from Burg to Alexander does not always present the same aspect throughout. On April 4th, 1881, he found that where it crosses the arm of the Lacus Somniorum, and passes from one range of mountains to the other, it did not look like a cleft, but resembled rather a high narrow wall, fringed with a parallel band of shade on its northern side. The cleft extending beyond this to the Mare Serenitatis was noted on March 20th, 1877, and has been frequently re-observed. On January 23rd M. Trouvelot traced a long, straight, narrow wall from the west side of a small mountain adjoining the southern border of Aristarchus to the western wall of Herodotus, beyond which it slightly projected in the form of a luminous point. [This bright spot probably pertained to one of the craterlets on the wall, which, when the interior of the ring-plain is in shadow, give a jagged

* According to Neison, a south-easterly continuation of this cleft beyond the mountain B was observed by Loder.

and serrated appearance to it.] What I presume to be the same object as that described by M. Trouvelot was observed by me on March 8th, 1884, and shown in my drawing of Aristarchus and Herodotus, published in the *Journal* of the Liverpool Astronomical Society. Its appearance at 8h. on that evening resembled a fine black line, as even as if drawn with a straight-edge, extending from a small circular depression on the south of Aristarchus to a craterlet (only half of which was in sunlight) on the western rampart of Herodotus. I failed, however, to see the luminous filament associated with it, and considered it as due to "a fault," or sudden drop in the surface. The next, and last, example of these curious features noticed is one which M. Trouvelot has frequently observed. It relates to what he deems to be a prolongation of the cleft Z (*Zeta*) pertaining to the Triesnecker system, which, commencing north-east of the latter formation, runs towards Rhæticus B, and terminates at the foot of a neighbouring mountain, near which point, according to him, the wall proper commences, curving round the east side of Rhæticus parallel to the border, and ending on the south at a little valley not far from a small crater between Rhæticus and Horrox. Where it crosses at right angles a number of deep ravines it resembled a fine thread of silver.

These observations, so far as they relate to the luminous walls, are so novel and remarkable, and have, moreover, so important a bearing on the conformation of lunar clefts, that it behoves observers to take the earliest opportunity of investigating the matter. As regards the connection between clefts and walls of rock forming apparent extensions of them, such instances are not uncommon, and were indeed noted by Mädler, as Dr. Klein remarks. A noteworthy example is found on the mountain barrier west of Mersenius, to which I specially drew attention in a recent notice of that ring-plain (*Selenographical Notes,* April, 1883), as affording an excellent illustration of the connection between a ridge and a cleft (ϕ *Neison's Map XV.*) With respect to the filaments of light observed by M. Trouvelot in Eudoxus and elsewhere, it is perhaps not very strange that such delicate and evanescent features have hitherto been unrecorded. We know that in some cases clefts are bordered by rocky walls, whose summits, when first lighted up by the sun, exhibit for a short time appearances very similar to those he alludes to. As instances in point I may refer to the cleft connecting Cardanus and Kraft, and to the wider portions of the great Serpentine valley north of Herodotus. The former, described in the *Selenographical Notes* for February last, when on the morning terminator is bordered on *both* sides by a narrow fringe of light,

a phenomenon only to be explained by supposing the existence, on one side at any rate, of a lofty wall or bank rising above the general level of the neighbouring surface.

Kempston, Beds: September 19, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER

THE STRUCTURE OF THE MILKY WAY.

I am surprised to have seen, nowhere, any discussion on the theories propounded, respecting nebulosity, by Mr. R. A. Proctor in one of some papers entitled "Needed Star Surveys," in *Knowledge*, some months ago; and which are well worthy of consideration, for they seem subversive of previous beliefs.

He holds, that all unresolved nebulous light, whether seen with the naked eye or with telescopes of any power, is either really nebulous matter or stars clustered far closer together in proportion to their size, than is the case in the firmament in general; grounding this opinion on the theory that, if stars are at a moderate distance apart and the observer's distance from them increases, they will get fainter, and disappear from sight, without taking the appearance of nebulosity. But it appears to me probable that a star impresses, not only a distinct point of the retina, but also more feebly a surrounding area, and that when it ceases to be visible *as a star* a considerable area is still perceptibly affected by it, the visibility depending, I believe, not only on the brightness, but also on the extent illuminated. If this be the case, then a number of stars not very close together, which are too faint to be visible individually, would still produce an appearance of milky light.

Nevertheless I conclude Mr. Proctor's theory is, to a great extent, true. If, then, the numerous whisps of milky light in the milky way are really nebulæ, whether resolvable or not, how is it that they are always, so far as I know, ignored in lists of nebulæ? Is this because they are so large, so complex, and so thickly strewn? Or is it because others hold a contrary opinion to Proctor's? Sir J. Herschel (vide "Outlines of Astronomy"

§ 798) must have done this, and so also Proctor himself, when he wrote "The Universe," for in the chapter on nebulæ he proceeds to prove that nebulæ are most abundant near the galactic poles, the very reverse of the fact if the newer theory be correct. He, however, states that *irregular* nebulæ are chiefly or altogether found in, or near, the milky way, so that this suggests a connexion between them and the whisps mentioned.

T. W. BACKHOUSE.

Sunderland: September 19th, 1885.

THE AUGUST METEORS.

During a total naked-eye watch of just over nine hours from the 5th to the 14th of last month, I saw 217 meteors, most of which—on the principal nights nearly all—were Perseids. Details as follows:—

1.	2.	3.		4.		5.		6.	
Aug.	h. m. h. m.	m.	m						
5.	9·41—10·51		26		6		14	Zermatt	5,300
6.	8·48—10·28	12	30	2	7	10	14	On the Riffel-berg	8,400
	13· 7—13·51	18		5		17			
7.	9· 8—10·38		42		7		10		
8.	9·49—11·15		43		11		15	Zermatt	5,300
9.	8·56—10·55	35	99	8	52	14	31·5		
	13·46—15·15	48		41		51			
	15·17—15·42	16		3		11			
10.	8·16—10·27	40	132	25	97	37·5	44	Visp	2,200
	10·45—12· 5	28		21		45			
	14· 0—15·16	56		47		50			
	15·16—15·25	8		4		30			
11.	9·23— 9·27		2		1		30	Aigle	1,550
12.	9·39—10·46	15	26	1	4	4	9	Geneva	1,200
	10·50—11·15	11		3		16			
13.	8·44—10·36	20	80	7	23	21	17	Dijon	800
	13·44—15· 3	60		16		16			
14.	8·40—11· 0	38	68	5	9	8	8		
	13·50—14·40	30		4		8			
	Total ...		548		217		24		

The second column gives the times of commencing and ending my watches (Paris time for Dijon, and Berne time for Swiss

stations); the next, the duration of the watches in minutes, corrected for the amount of cloud in the direction of observation; 4th and 5th columns contain the number of meteors observed, and their hourly rate for the intervals of time in column 3; then follow the place of observation and its height, in feet, above sea level.

In addition, I watched during 39 minutes on the 9th and 10th with a large pair of opera-glasses, but only saw one meteor therewith.

On the 9th were counted 17 meteors in 15 minutes, and 9 in 8 minutes; on the 10th, 18 in 15, and 13 in 6 minutes; too fast to more than denote their general direction, though the paths of many were secured for future mapping and deduction of a radiant point.

The great clearness of the atmosphere in Switzerland (except at Geneva) was particularly favourable for seeing a large number of shooting stars, and especially the fainter ones, but the area of sky visible was somewhat circumscribed by the mountains, this being least the case on the Riffelberg. There were no other decided hindrances except twilight from 15h. 17m. to 42m. on the 9th, and 15h. 16m. to 25m. on the 10th; and at Geneva, and to a slight degree at Dijon, the town-lights would reduce the visibility.

The number of brilliant meteors was small.

T. W. BACKHOUSE.

Sunderland: September 18th, 1885.

EUDOXUS.

Sir,—Those persons who do not take in M. Flammarion's monthly *L'Astronomie*, should beg, borrow, or steal (which, in the case of books, are all but equivalent terms) the number of that periodical for June last, as it contains a well-executed figure of the lunar crater Eudoxus, which is represented as being traversed by a very remarkable wall.

Should space be found for these lines in the *Astronomical Register* for October, the attention of observers may perhaps be called to Eudoxus, which, during several ensuing months, may be observed more favourably than has of late been the case.

I am, Sir, your obedient servant,

September 27, 1885. LUNA MENDAX.

NEW STAR IN ANDROMEDA.

Dear Sir,—The new star in Andromeda Nebula would appear from the various accounts, to have been first noticed by me on the 19th of August at 11 p.m., when I estimated it about 9½ mag., but cloudy weather came on and I had not any chance of observation until the night after I received Dun Echt Circular announcing the discovery (Sept. 3). The magnitude had then increased to about 7½. On night of 16th it appeared to be on the wane as it was of about 8½ magnitude.

Yours very truly,
ISAAC W. WARD.

NEBULA IN ANDROMEDA.

A remarkable change has taken place in the appearance of the well-known nebula in Andromeda, which would seem to have been produced by the outburst of a star nearly in the centre of that object. The discovery of the nebula itself is usually attributed to Simon Marius (Mayr) in 1612, who likened it to the flame of a candle shining through horn; but it is now known that it was noticed by the Persian astronomer Sûfi in the tenth century. It is visible to the naked eye, and has frequently been mistaken for a comet by those imperfectly acquainted with the heavens. The discovery of the appearance of a distinctly stellar nucleus was made by Dr. Hartwig at Dorpat on the 31st ult. Information of this being diffused by a Dun Echt circular, Mr. Common observed the nebula at Ealing on the 3rd inst., and comparing its appearance with that shown on a photograph taken by himself on the 16th of August last year, at once perceived that a variation of a very remarkable kind had really taken place. "The centre," he says, "of this nebula has hitherto always been noted as a bright condensation of the nebulous matter, looking [as described by Mayr] like a candle in a horn lantern, and so it appears in the photograph. Now it appears as an intensely bright point, entirely different in character, and very much more brilliant than before." Mr. Knobel, one of the honorary secretaries of the Royal Astronomical Society, observed it at Bocking, near Braintree, Essex, on the same night, and described the stellar nucleus to which Dr. Hartwig had called attention as being "conspicuously visible as a star of the eighth or ninth magnitude shining through the densest region of the nebula." No other change was apparent in the nebula itself, nor did the object suggest the idea to him of being really of the nature

of a nucleus thereto, but rather as if caused by a great outburst of light in a star not previously visible, similar to that which produced the appearance of the so-called new star near Cassiopeia in 1572, or the more recent phenomenon observed as having taken place in a star in the constellation Corona in 1866. Mr. K. Tarrant, of Pinner, indeed thinks that the central portion of the nebula was, so early as the beginning of July, much more condensed and bright than formerly. But it is in evidence that nothing like a stellar nucleus was visible up to the 9th of August. On the 19th of that month, however, Mr. Isaac W. Ward, of Belfast, reports having noticed it at about 11 o'clock at night, so that the outburst must have occurred between those dates. M. Lajoie saw it at Rheims on August 30th, one day before it was noticed by Dr. Hartwig at Dorpat on the 31st. The star is now of very nearly the seventh magnitude, not quite so bright, therefore, as that which appeared in the cluster 80 Messier (in the constellation Scorpio) in the month of May, 1860. It is not quite in the centre of the nebula, but about 1·6s preceding and 5″ south of the old nucleus (as estimated by Dr. Copeland, of Dun Echt), which is much overpowered by the light of the star. Mr. Maunder, of the Royal Observatory, Greenwich, examined its spectrum on the 4th inst. with the large half-prism spectroscope devised by the Astronomer Royal, and found it "of precisely the same character as that of the nebula, *i.e.*, it was perfectly continuous, no lines, either bright or dark, being visible, and the red end was wanting," so that, "there is at present no evidence of any outburst of heated gas, as was the case with the star T Coronæ in 1866, and Nova Cygni in 1876."—*Athenæum*, September 12.

REVIEWS.

The True Theory of the Sun. Showing the common origin of the solar spots and corona, and of atmospheric storms and cyclones, with the necessary formulæ and tables for computing the maximum and minimum epochs of solar activity, and the passages in time and place of the chief disturbers of the weather, from the equator to the poles in both hemispheres. By Thomas Bassnett, author of "Outlines of a Mechanical Theory of Storms." G. P. Putnam's Sons, New York: 25, Henrietta Street, Covent Garden, London. 1884. pp. xli. 263.

According to the author the ethereal medium, which he identifies with the "electric fluid," is in motion in mighty currents, straight, curved, and vorticose, in which are drifting the millions of suns with their retinue of planets, comets, and meteors—like rafts of floating débris on the bosom of a mighty river in time of flood. This infinite ocean of imponderable matter constitutes an infinite reservoir of *kinetic* and *potential energy*, and whilst the stars exert a feeble attraction on each

other, their proper motion is much more due to the irresistible currents of the ether. The idea of an inevitable catastrophe awaiting every sun and planet in the universe, by the extinction of its light and the loss of its heat, is worse than superfluous. It is fair to presume that the motion of the medium is the same as that of the planets at the same distances from the centre of the solar vortex, and this can be the case provided the density of the medium increases as the distance from the sun increases. Assuming that at the beginning the solar system was in a nebulous state, rotating around a common centre as disseminated particles, its rotation was similar to the rotation of the medium in the manner of a vortex composed of a series of vortices, the largest of which occupied the axis and centre of gravity of the whole nebula. Its own vorticose motion must rarefy the matter in the interior of the vortex. Our solar system, and by parity of reasoning, all other stellar systems, are placed in the axes of so many ethereal or electrical vortices, in which, while the laws of motion are rigidly observed, there may be an endless variety of phenomena. Questions will arise in the near future as to the effect which the motions of the ether have, not only on the undulatory theory of light, but on the law of gravitation, as affected by the ever-rushing stream of ether outwards from the sun; the admission of which is the *sine quâ non* to further progress.

It will be seen from the above, collected from various parts of the work, that the author's views of the hypothetic ether differ from what is commonly believed. So far from its being a fluid, its astonishing elastic properties are rather those of a solid, in the sense *that none of its elementary molecules are to be supposed capable of interchanging places*, or of bodily transfer to any measurable distance from their own special and assigned localities in the universe,* and every phenomenon of light points strongly to this conception. Again, whilst any resistance to the motions of planets, and comets of direct motion, is precluded by this theory, its author admits that the movement of retrograde comets presents at first sight a serious objection. This he endeavours to invalidate, but, we confess, we do not comprehend the explanation. Endued with planetary velocity at the earth's orbit the ether should be flowing at the rate of about 100,000 feet per second; a rather serious head wind, it would seem, against a comet of small mass.

A difficulty in reconciling his theory with Schwabe's table of the solar spots led the author, before the discovery of Neptune, to suspect the existence of another planet; and when Neptune was discovered there seemed no way of reconciling the two without another additional planet. This undiscovered planet caused an immense amount of work. The author does not pretend to have located it exactly, and he says there may even be two such planets, whose joint effect shall be equivalent to one in the position assigned. But, as there can be but little doubt that the solar spots are caused by the solar vortices, and these last made effective on the sun by the positions of the great planets, he has indicated a new method of determining the existence of all the planets exterior to Neptune. The deduced elements of the hypothetical planet for epoch January 1, 1883 are, $a = 47$, earth's distance unity .$L = 115°$ at epoch, $M = \frac{1}{7200}$. sun = unity, motion *retrogade*, orbit provisionally taken as circular, and inclination 0°. It is necessary, it seems, for this theory, that the motion should be retrogade, on which some explanations are offered, which, if valid, are by no means superfluous, for it involves the retrogade movement of the ethereal medium at that distance, and its

* Sir John Herschel, "Familiar Lectures on Scientific Subjects," p. 285.

being stationary before it changes from direct to retrogade motion. However, we are assured that the difficulty is not formidable, and are reminded that the whirlwind and whirlpool have their limits, and the cyclone is stated to have its anti-cyclone, so may a vortex have its anti-vortex ; and the future race of astronomers must have many unexpected surprises in store for them.

The axis of the sun seldom coincides with the axis of the vortex, which passes through the centre of gravity of the system, but by the conspiring action of the planets it may be drawn to a distance of even three semi-diameters from the centre of gravity.* This is essential to the theory, and occasioned the introduction of the theoretic planet. Its mass being only half that of Saturn, and its distance about five times greater, its power to displace the sun is, roughly, as 5 to 2. And if compared with the united influence of Saturn, Uranus, and Neptune, we find it still superior; for whilst these three are only competent to displace the sun 560,000 miles, planet VIII. displaces it over 600,000 ; so that as the displacement due to Jupiter alone is over 450,000 miles, it follows that the conjunction of these two planets, IV. and VIII., determines the periods of maxima and minima of the solar spots.

In connection with the above, and tables at the end of the book, are two large diagrams. The first represents the polar co-ordinates of the centre of gravity, of the solar system on the 1st of January of the years mentioned. Only Jupiter, Saturn, Uranus, and Neptune are taken into consideration, and the longitudes are computed for the mean equinox of 1850. This is an interesting diagram. Assuming it to be accurately constructed,† the centre of gravity was very near the sun's centre in 1851. In 1854 it was a semi-diameter from the sun's centre ; in 1842, 1843, and 1844, two semi-diameters ; and in 1883, 1884, rather less than two semi-diameters from the sun's centre. Such a representation might well be introduced into our astronomical books ; and if on a sufficiently large scale, the curve described by the sun's centre might be drawn. This the author has done in the second and larger diagram ; only we find introduced here the new planet to which he is sponsor, the effect of which is to remove the centre of gravity for 1884 to three semi-diameters from the sun's centre ; or if we take the amount from Table IV., at the end of the book, 3·131 semi-diameters.

In the conclusion of his work, the author, who claims also to have explained all cometary phenomena, and the true theory of the weather (for it seems that our own globe has its electric vortices, *terra-lunar*, seven in number; and meteorologists should see to this) thus recalls the principal features of his theory. 1. By rotation the solar vortex is rarefied, which originates a perpetual current of ether through the

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* As the quantity of matter in all the planets taken together is less than 1-700th of that in the sun, the extent of the curve described by the centre of the sun cannot be very great. The distance between the sun's centre and the centre of gravity of the system can never be equal to the sun's diameter, "Norton's Astronomy," p. 285. The combined effect of the great planets would remove the centre of the sun not more than 0·0085 of the radius vector of the earth from the centre of gravity, which is not so much as his own diameter. Mrs. Somerville, "Mechanism of the Heavens," p. 403. It will be found, we think, that on the supposition (which must be of very rare occurrence) of the four great planets being in conjunction, their combined effect would be to displace the sun's centre from the centre of gravity 932,200 miles, or 72,200 miles more than the sun's diameter, assumed to be 860,000 miles, and his distance 92,400,000 miles. This effect would be apportioned to the planets thus: Jupiter, 458,800 miles; Saturn, 251,700 miles; Uranus, 78,400 miles; Neptune, 143,300 miles. The effect of the lesser planets is trivial; that of the earth being only about 280 miles.

† We have tested it for January 1, 1884, and found it correct.

system. But, except at the centre (where the opposing currents meet, condense, give out their latent specific caloric, and otherwise by friction keep the sun at its present temperature, varying with the displacement), the ethereal medium is at its least density in the interior of the solar vortex. But in consequence of this current there is a central core of greatest density, and that density becomes *greatest* around the axis. So that the part of the vortex where the ether is *least* dense is several solar semi-diameters from the axis, and from thence the rarefaction diminishes, following the law of the square roots of the distances from the axis, or (which amounts to the same thing) the density increases outward directly in the same ratio. 2. That the solar spots are due to the displacement of the sun from the centre of the vortex, reaching their maximum when the displacement is at a maximum, and their minimum when the displacement is at a minimum; owing to the derangement of the sun's electric state by change of position in its orbit, as well as by change of position of portions of its equatorial zone by its rotation. 3. That the solar corona and pertuberances are due to the ether or electricity escaping from the sun, as well as to the sweep of the great polar currents, which last are rendered visible by passing through the attenuated matter carried up by the emanations from the body of the sun, and that these currents will account for all the peculiarities of the corona and for the zodiacal light. 4. That we have purposely treated the electric fluid as we would the air we breathe; its positive or negative condition being but a question of density or intensity of pressure and elasticity reducible to mechanical principles.

It will be remembered that Dr. Young, considering that the earth's passing through the ether might occasion eddies, affecting its undulations, and thus interfering with the law of aberration, proposed his famous hypothesis of the earth being as pervious to the ether as a grove of trees to the wind. On our author's theory this hypothesis is of course quite unnecessary; but Dr. Young would have had a hard task to meet the far more serious disturbances involved on the supposition of an ether abounding in vortices, eddies, and currents of immense rapidity, sometimes, too, flowing in different directions; an ether on which the stars are a "mere aggregation of chips as the drift of the Amazon collects in vortices and eddies in its passage to the Atlantic" (p. 205). Mr. Bassnett believes in the undulatory theory of light, and of course must think it reconcilable with all the enormous disturbances of the ether which his theory requires; or if not quite so in its present state, needing to be put in accord with it. We fancy the two are not to be reconciled, and that his views are opposed to what we do know of the nature of light. We prefer to abide by the beautiful theory, which has stood so many tests, of an elastic medium pervading all space, and all material bodies, tranquil and undisturbed except by the vibratory movements to which we owe light, heat, and more besides. Professor Stokes says, "We must remember that we have no direct evidence even of the existence of an ether. It is not directly recognizable by any of our senses; its properties may be, and doubtless are, very different from those of ponderable matter, and we must be content to learn them by degrees, as they may be revealed by the study of the phenomena which are referable to actions of the ether."* It is not by letting imagination run wild, and piling one extravagant hypothesis on another, that the difficult subject can be increasingly elucidated.

* Burnett Lectures on Light. By George Gabriel Stokes, M.A., F.R.S., &c. London. Macmillan & Co. 1884.

Although he employs many words, it is not always easy to gather the author's meaning; indeed, sometimes we have found it impossible. He seems to be an excellent person; and with many of the reflections interspersed throughout his book we cordially agree, whilst we regret to be unable to accept a theory which has cost him many years of thought and painstaking. We should rejoice if better judges can arrive, even partially, at a different conclusion. Anyhow, we are happy to find ourselves in accord with him on matters of far greater importance than any "theory of the sun," and we take leave of him wishing him better health, and every happiness in his declining years.

Annales de l'Observatoire Royal de Bruxelles. Nouvelle Série. Annales Astronomiques. Tome v. Seconde Fascicule. *Passage de Vénus du 6 Décembre*, 1882. Seconde Partie. *Documents et Observations.* Bruxelles : F. Hayez. 1884. pp. 130, and 2 plates.

This was the first scientific expedition organized in Belgium, and the perusal of this volume shows that it could not have been carried out by more able and conscientious observers. The stations were Texas and Chili. The members of the Texas mission were J. C. Houzeau, Director of the R. O., Brussels, Chief, and MM. A. Lancaster and E. Stuyvaert, of the R. O. The observations were made at San Antonio, and 124 micrometer measures were obtained. The *personnel* of the Chili mission was composed of MM. Louis Niesten, Chief, Charles Lagrange, both of the R. O., and Joseph Niesten, Capt. Comm. of Artillery. The place of observation was the garden of the Observatory of Santiago, the instruments of which were placed by M. Vergara, the Director, at the disposal of the mission. Here were made 606 micrometrical measures of the distance between the sun's centre and Venus. At the entrance, when about half the disc of Venus was upon the sun, M. Lagrange, using a telescope by Troughton & Simms, of 3½-in. ap., and power 160, saw Venus project behind it on the ground of the sky a long dark track equal to several diameters of the planet. He also saw the sun through Venus; the limb prolonged distinctly across the planet, and resembling the lunar crescent during the first days of a lunation. He gives sketches of these singular appearances, hard to explain on the assumption of their being objective realities. It will be remembered that Commander J. L. Wharton, R.N., saw an appearance like the first mentioned of the above, at Point Coton, Rodriquez, during the transit of 1874. He used an instrument by the same makers, 2¾-in. ap. and 160 power.

Publications of the Washburn Observatory of the University of Wisconsin, Vol. ii. Madison, Wisconsin, 1884. pp. v. and 347, Catalogue of Library, 53.

The Director of this Observatory, founded by C. C. Washburn, who died in 1882, is Professor Edward S. Holden. The Observatory has lately acquired a first-class meridian circle, by the Repsolds of Hamburg, of which a drawing is given in this volume. The object-glass, by Alvan Clark and Sons, is of 4·8-in. ap., and 57·6-in. focal length. There are two collimators, exactly alike. We observe that the investigation of their pivots, as also the determination of the thread intervals of the reticles of the meridian circle, were by Miss Alice Sanborn, student in astronomy. Prof. Holden observes that the Repsolds have an insuperable objection to glass reticles. One serious objection to them is that a dark field and bright wires cannot be obtained, but he thinks that in every other respect they are immeasurably superior to spider lines, which are three times as thick as the lines of the glass reticle, require

perpetual attention to keep them from dust, and are certain to be broken sooner or later. One glass reticle ought to last a hundred years. The circle is read by 8 microscopes, 4 for each pier. This fine instrument, described in much detail, is especially fitted for fundamental work; the telescope will show 9th magnitude stars in a bright field. There is no measurable difference between the pivots; they are, like nearly every other essential part of the instrument, very near to mechanical perfection. The instrumental constants of the circle and collimators have been determined very frequently during a period of about six months. Prof. Holden expresses his opinion freely as to the merits or demerits of the circle, indicating a few points in which improvements might easily be made. The cost of this instrument, every thing included, was £890 (4,274 dollars).

Besides other matter of interest contained in this volume, there is a list of 111 new double stars, and 2 nebulæ, encountered in sweeps; observations of 119 red or coloured stars seen in the same sweeps; occultations of 40 stars by the moon; star-gauges of Sir William Herschel, reduced to 1860, first and second series; counts of stars from the celestial charts of Dr. C. H. F. Peters and others; meteorological observations at Madison; list of auroras. An appendix contains the catalogue of the Woodman Astronomical Library.

The Washburn Observatory possesses a 15-in. equatorial; a portable transit instrument, two clocks, sidereal and mean time, the 6-in. equatorial that belonged to Mr. S. W. Burnham, who discovered with it some 400 of his double stars; and an auroral spectroscope is being made for observing auroras and the zodiacal light.

Our Sea-marks; a Plain Account of the Lighthouses, Lightships, Beacons, Buoys, and Fog-signals maintained on our coasts for the guidance of Mariners. By E. Price Edwards. With numerous illustrations. Longmans, Green, & Co. 1884. pp. 192.

This is a pleasantly written and interesting work on a very important subject. It has a good map, showing the ranges of the principal lighthouses and lightships on the coasts of the British Islands. From the list of the principal coast lights, corrected to March, 1884, the numbers of which correspond with those on the chart, we find that in England there are 118 coast lights; in Scotland, 64; in Ireland, 63; in the Isle of Man, 4; and in the Channel Islands, 7; making 256 in all. Price of the book is not marked.

DUN ECHT CIRCULAR, No. 97.

Professor Krueger telegraphs from Kiel, at midnight August 31, "Variation in Andromeda Nebula found by Dr. Hartwig (of Dorpat), starlike nucleus, please look for it."

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1885, September 1.

DUN ECHT CIRCULAR, No. 98.

A code telegram from Professor Krueger, of Kiel, announces that a suspicious object was discovered at [by ?] Brooks, on August 31, and was observed by Professor Pickering of Cambridge, U.S., as follows :—

Greenwich M. T.	R. A.	Decl.
1885, Sept. 2·7.	13h. 42m.	N. 36° 37′

The daily motions were *minus* 3m. 12s. and *plus* 19′.

It was found to be a circular comet, as bright as a 9th magnitude star, 2′ in diameter, with some central condensation, but no tail.

Respecting the new star in Andromeda, it is announced from Brussels that at the *commencement of August*, the stellar nucleus was not visible. Mr. Isaac W. Ward, however, reports having seen it on August 19th at 11 p.m. It was also seen at Rheims, by M. Lajoie, on August 30, and also independently by Mr. G. T. Davis, of Theale, near Reading, on September 1st.

The Dun Echt telescopes show it as a veritable 7½ magnitude star, with a fairly continuous spectrum. On September 3rd Lord Crawford and the undersigned found that the *Nova* is most probably situated some 1s·6 preceding and 5″ south of the old nucleus, which is much overpowered by the light of the star.

RALPH COPELAND.

Lord Crawford's Observatory,
Dun Echt: 1885, September 5.

THE PLANETS FOR OCTOBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	11 58 34	S. 2 12	5″·4	23 13·2
	9th	12 50 0	S. 3 49½	4″·8	23 33·1
	17th	13 33 46	S. 9 0	4″·6	23 51·4
	25th	14 22 49	S. 14 22½	4″·6	0 6·7
Venus ...	1st	14 56 31	S. 17 54½	14″·3	2 14·6
	9th	15 34 52	S. 20 49½	14″·9	2 21·4
	17th	16 14 18	S. 23 11	15″·7	2 29·3
	25th	16 54 35	S. 24 54½	16″·6	2 38·0
Saturn ...	1st	6 44 59	N. 22 18½	16″·8	17 50·5
	9th	6 35 54	N. 22 17½	17″·0	17 20.0
	17th	6 36 18	N. 22 17	17″·2	16 48·9
	25th	6 36 12	N. 22 17	17″·4	16 17·4
Neptune ...	4th	3 30 20	N. 17 10		23 18·3
	20th	3 28 31	N. 17 3½		22 19·0

Mercury rises about an hour before the sun, at the beginning of the month, the interval decreasing.

Venus sets, on the 1st, about an hour after the sun, the interval increasing.

Saturn rises about 9·30 p.m. at the beginning of the month, and afterwards earlier each night.

ASTRONOMICAL OCCURRENCES FOR OCTOBER, 1885.

DATE.		h. m.	Principal Occurrences.	Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m. α Pegasi
Thur	1	12 38 13 32	Occultation of λ Geminorum (3½) Reappearance of ditto			— 10 15·8
Fri	2		Sidereal Time at Mean Noon 12h. 45m. 28·21s.			10 11·9
Sat	3	7	Conjunction of Moon and Mars 5° 4′ N.			10 8·0
Sun	4	14	Conjunction of Uranus and Mercury 2° 1′ N.			10 4·1
Mon	5		Sun's Meridian Passage 11m. 40·24s. before Mean Noon			10 0·1
Tues	6	5	Conjunction of Moon and Jupiter 1° 25′ N.			9 56·2
Wed	7	19 31 8	● New Moon Conjunction of Moon and Mercury 0° 29′ N.			9 52·3
Thur	8			2nd Tr. E.	16 11	9 48·3
Fri	9					9 44·4
Sat	10					9 40·4
Sun	11	0	Conjunction of Moon and Venus 6° 23′ S.	4th Ec. D.	16 37 1	9 36·5
Mon	12			1st Ec. D.	16 51 38	9 32·6
Tues	13			1st Sh. E. 1st Tr. E.	16 16 16 50	9 28·6
Wed	14					9 24·7
Thur	15	13 20 22	☽ Moon's First Quarter Superior conjunction of Mercury and Sun Saturn's Ring: Major axis=42″·95 Minor axis=18″·41 Illuminated portion of disc of Venus=0·718 Illuminated portion of disc of Mars=0·913	2nd Tr. I. 2nd Sh. E.	16 7 17 48	9 20·8

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Pegasi
Fri	16		Sidereal Time at Mean Noon 13h. 40m. 39·94s.			9 16·8
Sat	17		Sun's Meridian Passage 14m. 38·61s. before Mean Noon			9 12·9
Sun	18					9 9·0
Mon	19					9 5·1
Tues	20			1st Sh. I. 1st Tr. I. 1st Sh. E.	15 52 16 32 18 10	9 1·1
Wed	21			3rd Sh. E. 1st Oc. D.	15 55 16 10	8 57·2
Thur	22			2nd Sh. I.	17 31	8 53·2
Fri	23	9 22	○ Full Moon			8 49·3
Sat	24	15 0 16 10	Occultation of B.A.C. 987 (6½) Reappearance of ditto	2nd Oc. R.	16 3	8 45·4
Sun	25					8 41·4
Mon	26	10 3	Near approach of B.A.C. 1526 (6)			8 37·5
Tues	27	12 0 13 6	Occultation of B.A.C. 1930 (6½) Reappearance of ditto	1st Sh. I. 1st Tr. I.	17 45 18 32	8 33·6
Wed	28	5	Conjunction of Moon and Saturn 4° 8′ N.	1st Ec. D. 3rd Sh. I. 4th Oc. D. 1st Oc. R.	15 7 35 16 34 18 0 18 10	8 29·6
Thur	29	10 5 10 26	Occultation of 1 Cancri (6) Reappearance of ditto	1st Tr. E.	15 19	8 25·7
Fri	30	5 57	☾ Moon's Last Quarter			8 21·8
Sat	31	16	Conjunction of Moon and Mars 4° 16′ N.	2nd Oc. R.	18 48	8 17·8
NOV.						
Sun	1					8 13·9

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
Oct. 1	— 8 5 E.	Oct. 11	+50 2M.	Oct. 21	—71 49M.
2	20 16	12	37 50	22	83 59
3	32 28	13	25 40	23	+83 50 E.
4	44 39	14	13 28	24	71 39
5	56 51	15	1 16	25	59 28
6	69 2	16	—10 54	26	47 17
7	81 13	17	23 5	27	35 7
8	+86 35M.	18	35 16	28	22 56
9	74 24	19	47 27	29	10 46
10	62 13	20	59 38	30	— 1 25
				31	13 36

Moon nearest the Earth, Oct. 3, 11h.
,, farthest from ,, ,, 15, 18h.
,, nearest the ,, ,, 28, 8h.

Books received:—Sidereal Messenger.—Astronomische Nachrichten.—Traité de la Lumière, &c., avec un Discours de la Cause de la Pesanteur. Par Christian Huygens. Edidit W. Burckhardt. Lipsiæ. Gressner & Schramm.—Bulletin Astronomique. Tome II. Paris: G. Villars. 1885.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

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The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, 11, *Angel Court, Throgmorton Street, E.C.*, **not later than the 20th of the Month.**

The Astronomical Register.

No. 275. NOVEMBER. 1885.

THE LIVERPOOL ASTRONOMICAL SOCIETY.

The opening meeting of the session, 1885-6, was held on Tuesday, 13th Oct., 1885, in the Association Hall, Mount Pleasant, Isaac Roberts, F.R.A.S., F.G.S., the President, in the chair. There was a good attendance. Among those present were Messrs. R. C. Johnson, F.R.A.S., W. H. Sharp, H. Thomas, and J. W. Appleton, F.R.A.S. (Hon. Sec.), besides Mr. W. H. Fleming, B.A., F.R.A.S., of the Royal Observatory, Cape of Good Hope, who is on a visit to this country. In his opening remarks the President said that the Society, during the last two years, had been very successful; the number of members now exceeds 200, many of whom are non-resident in Liverpool. The Society included many hard and careful workers in astronomy; and we may congratulate ourselves that it is now well known throughout the world. The chief astronomical event of the year had been the appearance of a new star in the nucleus of the nebula in Andromeda. On the 3rd September he examined the nebula with his 7-inch refractor, by Cooke, and photographed it with his 20-inch reflector, by Grubb. The refractor showed the star-like nucleus very sharp. The spectroscope showed the well-known dull continuous spectrum, without any bright lines. Seven photographs were taken on the same day. The photographs show clearly that the star and the nucleus are not coincident. The whole nebula is about 3′ of arc in diameter, is a nearly circular patch, with an undefined boundary, the star being at a little distance (about its own apparent diameter) to the south-west side of the nucleus. Other photographs confirm this description. The

drawings of this nebula by Argelander, Bond, Espin, and Common differ widely from each other. Mr. Common's drawing of the nebula, published in *Nature* on the first of this month, corresponds with his (Mr. Roberts') own photographs, where the nucleus appears as a slightly oval patch of light. That drawings made from eye observations should differ widely we have ample experience, but it is disappointing to find photographs differing. The different instruments employed, the sensitiveness of the plates, the time of exposure, and the state of the atmosphere, may account for some of this; but we may assume that the nebula itself has changed. If these photographs, varying as they do, are not misinterpreted, these changes are of a startling character, and the questions involved are of great importance to astronomers.

W. H. St. L. Gage, Esq., F.R.A.S., also contributed a paper on the same subject, ably arguing that the new star is in the nebula itself, and the following are the conclusions at which he arrives:—

1. That it is highly probable, nay, almost certain, that the nova is in the nebula.

2. If optical effect is of any value, that it is *far down* in the nebula.

3. That it is one second preceding the present nucleus, and slightly south of it.

4. That its colour is bright orange, though its variation is probable.

5. As regards the nebula itself, that its nucleus has brightened, and the whole object is brighter than formerly.

6. That a great change has taken place in the nebula, the exact nature of which is uncertain; but the so-called "star" differs in many essential respects from what generally goes by that name.

R. C. Johnson, Esq., F.R.A.S., remarked that the nebula is singularly non-actinic; and urged the necessity that its shape and size be truly got at, for if it changes as much as has been supposed, many important questions are suggested respecting it.

Mr. Finlay gave an interesting account of the photographic charts of the sky that they were taking at the Cape. This idea had been taken up in Paris, as well as by the Liverpool Astronomical Society. At the Cape they used a 6-inch lens with 5-ft. focal length, but had ordered a 9-inch lens, which would soon be ready. The whole southern hemisphere will be divided into 60 squares, each one overlapping the one adjoining it. Two plates are being taken of each picture, so as to avoid mistake. The length of exposure is one hour. The work is expected to

take from three to four years, and will comprise 1,000 pictures. So far the work has been very successful, the images being very good and round, and the doubles very clear in a magnifying glass; but the work of measuring has not yet been begun. The electric light is being used for the microscopes.

A paper by J. Rand Capron, Esq., F.R.G.S., *On Celestial Photography*, was read; and one by Mr. Fleming, giving an account of a lunar halo and paraselenæ, visible at Kirkwall; and other interesting and valuable papers were contributed.

ON THE GREAT NEBULA OF ANDROMEDA.

(Translated from No. 2687 of the *Astronomische Nachrichten*, columns 391 and 392.)

The following interesting contribution to the history of the Great Andromeda Nebula, is by Herr C. V. L. Charlier, who writes from Upsala under date of 22nd September:—

" My attention has been directed accidentally to an old work upon this nebula, whereby an interesting light has been thrown upon the phenomena recently witnessed therein. The work in question is written by the French astronomer Boulliaud, and its complete title is as follows:—'Two Notices to Astronomers. By Ismael Boulliaud. The former concerning the new star which was seen a few years ago in the neck of the Whale; the latter concerning the Nebula which appeared anew, two years ago, in the northern part of the girdle of Andromeda. Paris, 1667.'* In this work the author commences by saying that when astronomers at the end of 1664, directed their attention to the great comet of that year, a phenomenon of no less interest presented itself to them, to wit the Nebula in Andromeda. He goes on to state that this nebula had been observed also earlier twice, namely by Simon Marius (who is usually held to be the first person in Europe to have observed it) in 1612; and by 'a certain person, name unknown,' who about the year 1500, entered it by means of dots upon a map of the constellation of

* I am quite as unfamiliar with this treatise as Argelander was (see Bonn Observations, Vol. VII. p. 324). In the Philosophical Transactions for 1666, pp. 381-383, there is an extract therefrom. Furthermore, in the volume for 1667, p. 459, there is the following notice by Boulliaud:— " Anno. 1667, in January, when the clouds and Misty weather which had continued for a good while, did permit us to observe, the star *Nebulosa* in the *Girdle of Andromeda* (which may well enough be seen by the bare eye) appeared much obscurer than the year before. In the months of *February* and *March*, I did not see it."—*Krueger.*

Andromeda. After expressing his great astonishment that Tycho Brahe did not observe the nebula, the author says, 'Since it was observed neither by Hipparchus, nor by any of the ancient observers; nor, indeed, in the last century, by Tycho, the prince of astronomers, nor by the venerable Bayer; and since *in this month of November*, 1666, *in which we write, it appears very much diminished and dull, after having shone with extreme brilliance two years ago,* it follows, of necessity, that it is seen bright and dull by turns.'" *

Reasons may possibly be assigned how it was that the nebula was not observed either by Hipparchus or by Tycho Brahe, but the decided testimony of the author to the effect that the nebula in November, 1666, appeared very dull after it had shone out brightly two years before, points to the occurrence also in the year 1664, of a phenomenon similar to the one which engages so earnestly the attention of the astronomers of the present day.

SELENOGRAPHICAL NOTES, NOVEMBER, 1885.

By Thos. Gwyn Elger, F.R.A.S.

Taruntius. This fine ring-plain, notwithstanding its size and the remarkable character of the details in its vicinity, has been unaccountably neglected by observers, though it might well form the subject of a monograph on a large scale, if any one could be found willing to devote the time and care required for preparing it. On September 26th, 9h. 30m. to 10h. 20m., it was well placed for observation under evening illumination, the east longitude of the terminator being about 52°. At this time the most noticeable feature on the ring was the brilliant crater C, which marks the northern termination of the loftier section of the eastern wall. Beyond this, towards the west, is another much smaller crater, also on the site of the wall, which is not shown in the maps. A third crater, not always an easy object, stands on the summit of the south-eastern border. Under a high light its place is pointed out by a very brilliant spot, and in the early stages of evening illumination by a well-marked notch or indentation in the wall, a circumstance which renders its omission by Mädler and Neison strange, especially as they show the much more difficult and delicate gap close by in the south wall. There are also two good sized craterlets on the

* This paragraph, between the inverted commas, is given by Herr Charlier in the original Latin.

outer slope of the north-west wall of Taruntius unrecorded, though they are obvious enough under oblique light. The conditions which prevailed were especially favourable for displaying the great extent and noteworthy contour of the gently-sloping glacis of the ring-plain. On the south it can be traced to a distance of at least 30 miles from the border, its extreme southern limit being marked by a bright crater nearly as large as C on the north-east wall. There are many interlacing low ridges and spurs on the western side of this slope. On the east and west the glacis, though wide, is narrowed to a breadth of 10 or 12 miles, while the low broken southern border shows scarcely any indications of a bank or slope, except in the form of a large mound north of the crater C, though there are two prominent ridges running from it towards the north. On the plain east of Taruntius there is a dusky annular formation (marked *l* by Mädler and in Neison's Map I.) with a smaller ring attached to it on the south, and still further to the east is a curious zigzag valley with very brilliant flanks, reminding one of portions of the great serpentine cleft near Herodotus. A few miles north-west of the ring-plain are two remarkable little mountains, from which runs a prominent cleft to a bright crater situated on the southern edge of the mountain region bordering this side of the Mare Crisium. This object is not shown in Neison's Map, nor referred to in the text, though it has all the appearance of being a true cleft as distinguished from a narrow valley. The ridges on the floor of Taruntius concentric with the border are among the most interesting features of the formation. They form, with a few short breaks, a continuous ring, though it is not always possible to see it complete, as portions of it are very low, and become confounded with the general surface of the interior under a high light. The best view of these ridges is perhaps obtained about the time when the east wall of Atlas and Goclenius are on the morning terminator. The ring is then seen nearly entire, except on the south-west, where it is obscured by the shadow of a lofty part of the wall. At this time, also, two little mountains are visible upon it, due north of the crater C. Under a high light this inner circle is indicated by a continuous light marking, always visible in good air.

Geminus. Between 9h. and 10h. on September 25th, this object was favourably situated for scrutiny, the eastern longitude of the evening terminator being about 64°, and the shadow of the eastern border occupying nearly one-fourth the apparent area of the floor. In Neison's Map IV. a conspicuous cleft (marked zeta) is shown, running in a south-westerly direction

from a crater on the southern glacis of Geminus. This was plain enough, but an extension of it traced through the border and across the southern portion of the interior as far as the shadow of a peak on the eastern wall is not drawn. At times of best definition I noted a faint short branch of this cleft running up to the southern border. On the north side of the floor there is another cleft of a very similar character, running from the bright mountain which marks the extreme northern boundary of the ring, nearly up to the inner slope of the west wall. Both these objects trend from north-east to south-west, and, though they are not difficult at this stage of illumination, I can find no records of them. There is a peculiarity in the colour of the surface surrounding Geminus, which calls for remark. It is especially noticeable in the hilly region on the east of the formation, and has often attracted my attention, though never more strongly than on the 25th of September. Even with a moderately high magnifying power the surface is unmistakably brown, and the greater the amplification the more evident it becomes that no other colour will truly represent the actual tone of the region hereabouts at this stage of illumination. This is not altogether an exceptional case, as a similar colour has been remarked in other districts near the west limb. The phenomenon is commended to the notice of those who cherish the idea of "meteoric dust" as being explanatory of lunar surface tints.

Kempston, Beds.: October 21, 1885.

THE LONGITUDE OF SYDNEY.

(Abridged from the *Sydney Morning Herald*, February 21, 1885.)

Mr. Tebbutt had deduced a value for this important element in 1880, from lunar occultations of well-determined stars, the tabular places of the moon being corrected by contemporaneous observations made at Greenwich, Radcliffe, and Washington. The result obtained from 23 occultation phases observed at Windsor was 10h. 4m. 50·64s. E. of Greenwich, the telegraphic longitude of Sydney east of Windsor being adopted as 1m. 28·83s. He says, "I pointed out that this was, so far as I was aware, the first attempt to deduce Australian longitudes from the occultation method, which I then regarded as the most satisfactory, though at the same time the most laborious, of all the purely astronomical methods." Two series of observations of occulta-

tions of small stars at the moon's dark limb had been made by Mr. Tebbutt, with all possible accuracy. They embrace the periods 1864-70, and 1873-76. The first period comprises 78, and the second 81 occultation phases. Prof. Auwers, of Berlin, has now turned the second series to account in conjunction with 15 occultation phases, observed at Melbourne at the time of the transit of Venus, in 1874. Notwithstanding the imperfect character of the instruments employed at Windsor, the transit and equatorial instruments having clear apertures of two and four and a half inches respectively, Prof. Auwers has expressed his sense of the high value of the series of observations, both on account of their number and of their individual accuracy. The resulting values of the individual longitudes are remarkably consistent. The longitude of the Sydney observatory, as deduced from 75 disappearances at the moon's dark limb at Windsor, is, according to the author, 10h. 4m. 50·08 E., and from ten disappearances and two reappearances at the same limb at Melbourne, is 10h. 4m. 48·55s. By assigning proportional weights to these results, according to the number and value of the observations from which each was obtained, he gets 10h. 4m. 49·82s. E. as the final longitude of Sydney, from the occultations. Combining this result with the values derived by Messrs. Scott and Russell directly from moon culminations at Melbourne, he arrives at 10h. 4m. 49·60s. E. as the definite longitude of the Sydney observatory, from absolute methods. Taking the mean of the telegraphic determinations of difference of longitude of Sydney and Melbourne in 1868 and 1883, he makes the longitude of Sydney by telegraphic operations between Greenwich and Sydney to be 10h. 4m. 49·26s. E., or only 0·34s. less than that derived from the absolute methods. Mr. Tebbutt remarks "Astronomers have, therefore, reason to congratulate themselves on the close agreement of the results obtained by telegraphic and purely astronomical methods. The fundamental longitudes for Australia by the absolute methods are, according to Prof. Anwers:—

		h.	m.	s.	
Sydney	...	10	4	49·60	E. of Greenwich
Melbourne	...	9	39	54·17	,,
Adelaide	...	9	14	20·42	,,
Windsor	...	10	3	20·77	,,

And these results will, it scarcely need be stated, have a most important bearing on astronomy, navigation, and the operations connected with the trigonometrical survey of the colony."

These determinations are indeed eminently satisfactory. It will be seen that the longitude by the 12 Melbourne observations

was correct to about one second. The value obtained by Mr. Tebbutt from his first series of 23 occultations, differs only 0·56 sec. from that obtained by Prof. Auwers from the series of 75 occultations. If the probable error of the first series had been assumed to be one second, $23 \times 4 = 92$ would be the number of observations required to reduce that error one half (since the probable error diminishes as the square root of the number of observations increases). The square roots of 23, 92, and 75, being respectively 4·8, 9·6, and 8·7, the last is found to reduce the probable error to 0·55 sec., nearly the same as found by observation. Taking the length of a second of arc at the latitude of Sydney, $33^\circ\ 52'$· S. to be = 84·3 feet, a second of time would be 1264·5 feet, so that if we assumed that the longitude as now determined is correct to a tenth of a second, it would be true to about 126 feet. The telegraphic discrepancy of 0·34 sec. or $5''\cdot 1 = 430$ feet.

The best thanks of astronomers, navigators, and geographers, are due to Mr. Tebbutt, the Melbourne observers, and Prof. Auwers for these important fundamental longitudes. Here, as so often in astronomy, a few figures represent many nights and days of patient and skilful work. The assigned longitudes of places, even of some importance, have required to be corrected in late years. Thus the longitude of the Lisbon observatory was found to be 8·54 sec. too little,* and that of the Madras observatory 2·1 sec. also too little.

MUSEO COPERNICANO.

The Copernican Museum was founded by Dr. A. Wolynski, a Pole, author of various works on Copernicus and Galileo Galilei, who took refuge in Italy after the Polish insurrection of 1863. He presented a collection, made in ten years, of a number of objects in relation to Copernicus (who passed most of eight years in Italy), to this museum in Rome, which Padre Denza, the President, and the members of the Meteorological Congress, have now petitioned the King of Italy to take under his patronage. Amongst the objects collected, both in Poland and elsewhere, and given by the exiled countryman of Copernicus, are over 500 bound volumes; 102 medals in gold, silver, and bronze; 107 silver coins; 18 pieces of sculpture in marble, bronze, terra-cotta, &c.; 33 paintings in oil, and prints; 17 scientific instruments, and furniture and other articles.—From *La Nazione,* Sept. 15th, Florence.

* See *The Observatory,* Vol. III., p. 455.

EXPEDITION TO OCEANIA FOR THE OBSERVATION OF THE TOTAL ECLIPSE OF THE SUN OF MAY 6, 1883.

(Continued from page 236.)

We come next to the report of M. Palisa to the chief. M. Palisa was charged with the search for intra-mercurial planets. He had prepared a chart containing all the stars as far as the 7th magnitude 15° to the west and the east of the sun, and 3° north and south of the ecliptic. This he changed for another given him by M. Holden, chief of the American expedition, which was lithographed, and on a smaller scale. The French Commission, at the suggestion of its president, had decided to divide the observation between MM. Trouvelot and Palisa, and it was agreed that each observer should devote at the least three minutes to the exploration of the region allotted to him. The exploration of the east side fell to M. Palisa. He employed a sweeper by Merz, 4 feet 7 inches focal length, and 6·3 in. aperture, mounted parallactically. The power was 13 times, and the field 3°. He began by sweeping from the sun in the direction of Saturn, first towards the south, and when he found no stars he went back to the sun and then more towards the north. He gives a list of 9 stars, from mag. 4·2 to 6·0, the places of which are noted in the Bonn Catalogue. When the fourth minute of totality arrived he desired to make a search on the other side, and found there a star that appeared to him to be brighter than as marked in the chart. He took its position, but found that it was one of the Bonn Catalogue. After this operation totality was over. The upshot of his observations was that within the limits of the regions (Equinox 1855·0) 2h. 52m., between +14° and +19°, as far as 3h. 40m. between +16° and +22°, there was no star of the 5th magn. which was not marked in the chart. He also observed the times of the contacts.

There follows the summary of observations by M. E. L. Trouvelot. The instant of first contact was not noted, the moon having advanced a very appreciable distance on the sun's disc when first seen. M. Trouvelot found it very easy to trace the moon's limb on each side of the solar crescent, and outside of the sun to a distance of about 3′, the borders of the moon being projected on a pale light. This phenomenon was more or less apparent during the greater part of the partial phase. Shortly after, small luminous brushes seemed to proceed from the two angles made by the intersection of the limbs of sun and moon. These diverged as they extended, and rendered the edges of the moon beyond the sun very easy to be seen. The moon did not

appear flat, like a disc, but inflated, like a sphere. An hour later the extremities of the cusps of the thin solar crescent were all at once in rapid vibratory movement, and exhibited the prismatic colours. At this moment the extremities of the cusps appeared in the two telescopes greatly rounded off. Two minutes after this a large band of deep red colour, well defined, surmounted by another grey one, very faint, and of equal breadth, formed and surrounded the thin solar crescent on all sides. "This phenomenon," says M. Trouvelot, "which was evidently optical, caused me much disappointment, for I saw that these coloured bands must needs mask the important and delicate phenomena which occur at the moments of the second and third contacts. Five and a half minutes more the corona was visible, and very plain on the border of the moon not yet advanced on the sun. It extended to a certain distance from the dark limb, and gradually faded off. The observation of the second contact with the 6-in. telescope, and power of about 150, left nothing to be desired. My first impression was that the corona was less brilliant than in the eclipse of 1878, which I observed in America, but its structure was much more complex. I consider the drawing I made correct, at least, in the more characteristic features. The corona showed a remarkable rift on the east. On the west it displayed a kind of whitish tuft that corresponded to a group of prominences observed with the spectroscope a little before the eclipse."

The search for inter-mercurial planets, M. Trouvelot thus describes:—"After two minutes and some seconds devoted to the study and drawing of the corona, I began to explore the regions west of the sun, as agreed upon with MM. Janssen and Palisa. I set the telescope 10° of declination to the north of the sun's centre, and slowly swept the sky from east toward west for a distance of 15° in R.A. The first sweep brought into the field a small faint white star. Two other sweeps were made with no result. The fourth sweep brought into the field a bright star of deep orange colour, estimated as of 4 or 4·5 magn. Whilst endeavouring to bring this star into the very confined field of the 6 in. eye-piece, in order to see whether it showed traces of a disc, some disorder between the two assistants at the hour and declination circles prevented my succeeding. The approximate place of this star was a little north and west of the sun. Whilst occupied with it, the helmsman, Michael Guillaume, who was seated behind me engaged with the chronometer, exclaimed that he saw the star with the naked eye. During my search for planets I heard M. Janssen calling me to confirm the sight of the dark lines of Fraunhefer, which he said were very strong, and

readily seen; but I could not comply with his invitation without being wanting to my programme. After some more very limited sweeps, which only brought to view a faint white star, I was warned that five minutes were already run out, and that I must prepare for the observation of the third contact, of which I gave the signal, but soon perceived I was mistaken, and had taken the edge of the luminous tuft above mentioned for the real edge of the sun. I at once apprised those who were noting the time by the chronometers, and gave the moment of real contact (two seconds later), but doubtless a little too late. The coloured bands produced around the solar crescent, of which I have spoken above, did not allow of my seeing either chromosphere or prominences. During the total darkness, L. Hamy, sailor of the *Eclaireur*, identified and marked with a pencil, on a star-map which I had prepared beforehand, the places of twelve stars and planets, which were visible to the naked eye during totality. I should very much have desired to put to the proof an idea which I briefly indicate here, which is to determine with the spectroscope whether the corona has a rotary movement like the sun, or proper movements of any kind. I had, previous to our departure, prepared an apparatus for this purpose, but the already numerous observations which I had to make during the total darkness obliged me to give up my project.

"M. Janssen gives the results of the observations under several heads.

"1. Intra-Mercurial Planets. We see, by the observations of M. Palisa, that in the eastern region, which he had to explore, no star was visible of a magnitude higher than the 5h. He also turned his eyes to the western region, and saw there an object that he recognised to be a star. Considering the extreme skilfulness of M. Palisa in this sort of search, it is impossible not to attach the highest importance to his negative conclusions. M. Trouvelot arrives at a less clear result for the western side, but we know that this distinguished observer desires to review the region where the sun was at the time of the eclipse, before giving his opinion. M. Holden, chief of the American mission, examined all the circumsolar regions, and at our request, the western region more especially. This learned astronomer also arrives at a negative conclusion regarding the existence of intra-mercurial planets. On the other hand, when we reflect that the objects indicated by M. Watson in 1878, may, within the limits of the errors incidental to the method employed by that astronomer, be identified with two stars in Cancer, we arrive at the conclusion that at the present day there is very little probability that one or more planetary bodies of any importance

exist between Mercury and the sun. In a note M. Janssen adds, 'our photographs, although not yet examined in a complete manner, appear to lead to the same conclusion;' and he says, 'In this respect we consider that the French and American missions have greatly helped to clear up one of the most important problems of the solar system.'

"2. CONTACTS. The corrected times for the observations of M. Trouvelot give, for the second contact, or commencement of totality, 23h. 31m. 51·8s. meantime at Caroline. For the third contact, or end of totality, 23h. 37m. 15·8s. meantime, Caroline. The difference, 5m. 24·1s., gives the duration of totality, according to M. Trouvelot's observations. According to M. Tacchini, we have 5m. 23s., a satisfactory agreement. According to the report of M. Trouvelot, the end of totality was noted rather too late; which would involve a duration a little shorter, and reconcile still more the two results.

"THE CORONA. The report of M. Tacchini testifies that that able astronomer has made some remarkable observations at Caroline, notably, in that which relates to the analogy of the constitution of the spectrum of certain parts of the corona with the spectrum of comets. It was in my own programme to examine this analogy. It is a point which should be very carefully verified in the next eclipses. For the rest, I leave to M. Tacchini the care of developing himself the observations he made. It is seen by my report, that the chief object of my observations was to decide a point in the constitution of the spectrum of the corona, that has always seemed to me very important—that is, to ascertain whether the light of the corona contains an important proportion of the solar light. The result has in this surpassed my expectation. The Fraunhofer spectrum, so complete, of which I was witness at Caroline, proves that, without denying that a certain part is due to diffraction, there exists in the corona, and particularly in certain parts of it, an immense quantity of reflected light; and since we know besides that the coronal atmosphere is very rare, there must be found in those regions cosmic matter in the state of solid corpuscles, in order to account for that abundance of reflected solar light. The more we advance, the more we see the complexity of the constitution of those immediately circumsolar regions; thus it is only by persevering and very varied observations, and a very full discussion of those observations, that we shall be able to arrive at a thorough knowledge of those regions. In this point of view the great eclipse of 1883 has enabled us to take a new step in advance.

"PHOTOGRAPHY OF THE CORONA. The result of the examina-

tions of the photographs will be given at a later period; for they indicate several very interesting phenomena, and require an exhaustive study. I will only say at present that these photographs show the corona of greater extent than as viewed in telescopes, and that the phenomenon appeared limited and fixed during the whole period of totality. I had prepared a photometric measurement by photography of the LUMINOUS INTENSITY OF THE CORONA. This experiment shows that at Caroline the illumination given by the corona was greater than that of the full moon. The exact figures will be given hereafter. It must be observed that this is the first time that a precise measure of the luminous intensity of this phenomenon has been made."

M. Janssen's report is accompanied by a photograph of the corona, which has neither been retouched nor enlarged. It was taken with an objective of 8-in. aperture and 47-in. focal distance. The exposure was continued during the whole period of totality (about five minutes). The image of the sun is about four-tenths of an inch in diameter.

The return voyage was happily effected. The *Eclaireur* took up the party on the 13th May, and brought them to Tahiti, where they were cordially received by the Governor, the Director of the interior, and the inhabitants, and where they visited the spots of chief interest, especially Point Venus, a place still replete with remembrances of Cook. At Paea they were received by the Tahitian chiefs after their ancient usages. M. Janssen was laid by owing to the fatigues of the Caroline expedition, and missed, in consequence, some of the festivities amongst the military and civil circles in honour of the party. He says, "Tahiti will ever be the most charming *souvenir* of their voyage." The next place should have been San Francisco, but hearing that the Island of Hawaii was then exhibiting important volcanic phenomena, M. Janssen requested the Captain of the *Eclaireur* to put in there, which was seconded by the Governor. As it was, touching at the Sandwich Isles enabled a supply of coal to be taken in; which was the means of rather accelerating their arrival at San Francisco. At Hawaii, M. Janssen repaired to the crater of Kilauea, the most remarkable in the world, and spent a night in it. On the borders of a molten lake of lava, his investigations led him to perceive some singular analogies between these volcanic phenomena and those on the surface of the sun. He also made the spectrum analysis of the flames issuing from the lava, and established the presence of sodium, hydrogen, and carburetted combinations. Lastly, he made for the home establishments a collection of minerals, and of specimens of gas, under the circumstances always of interest.

Before crossing over America from San Francisco, M. Janssen, in company with M. Trouvelot, paid a visit to the Observatory of Mount Hamilton, which will possess the largest telescope in the world. He visited, afterwards, the Observatories of Madison, Chicago, Washington, and Cambridge, where are found large and famous instruments, which had for him the highest interest. The party received from all the American savants the most flattering and cordial welcome. On the 15th of August the steam packet *The Canada*, of the Transatlantic Company, left New York with them, and brought them back safely to France.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

RED SPOT ON JUPITER

SIR,—The following observations have been obtained here :—

1885.		Red Spot on Central Meridian. h. m.
October 24	... ...	17 32
„ 26	... ...	19 10

The spot is very plain, though not so dark as the equatorial belts, or nearly so conspicuous as it was some years ago.

The Ephemeris which I gave in the *Register* for September, pp. 216-17, appears to be correct within a few minutes.

Yours faithfully,

Bristol: October 27, 1885. W. F. DENNING.

REVIEWS.

In *Ciel et Terre* for May 15th, 1884, is an article, which deserves attention (by M. J. C. Houzeau), on the problematical planet Neith, with an orbit nearly the same as that of Venus. Another valuable article is by E. Lagrange, on the morning and evening twilights. According to M. Hellmann the following propositions result from his observations: 1. The distance of the sun below the horizon at the end (or at the commencement for the dawn) of the phenomenon is not a constant. 2. There is an annual period with a maximum in winter and a minimum in

summer. 3. The angle of depression is greater at the dawn than at the evening twilight. 4. There is a close connection between the hygrometric state of the air, and the value of this angle which increases and decreases with the former. This angle has recently been given as follows: 15° 9′ mean for Athens (J. Schmidt); 15° 6′ Atlantic (Behrmann); 15° 6′ South of Spain (Hellmann); 15° 28′ Spain, dry season, and 16° 32′, rainy season (Hellmann). A long series of comparative observations of morning and evening twilights show that the sun's depression is greater in the morning than the evening. The air, in fact, is usually more saturated in the morning than in the evening. Some of these results are exhibited in the following table:

1877.			Angle of Depression.		Hygrom. State.
			° ′		°
March 6	Morning	...	18 15	...	72
„	Evening	...	15 51	...	55
„ 7	Morning	...	17 51	...	80
„	Evening	...	16 3	...	61
„ 8	Morning	...	17 15	...	82
„	Evening	...	15 15	...	77
„ 12	Morning	...	19 13	...	84
„	Evening	...	15 24	...	61
„ 13	Morning	...	19 25	...	83
„	Evening	...	15 26	...	74
„ 14	Morning	...	18 13	...	88
„	Evening	...	15 50	...	68

These observations made at the Observatory of San Fernando, near Cadiz, also confirm the annual variations of this angle, a discovery due to Schmidt and Hellmann.

The Great Comet of September, 1882.—We make the following extracts from a paper by Professor H. A. Howe, Denver University, U.S., in the *Sidereal Messenger*, May, 1884. "This great comet has fled from the gaze of man, and thirty generations of astronomers will pass away before it will submit itself to human scrutiny. Then, perchance, it will again burst unexpectedly into view, to be firmly bound by the chains of mathematical analysis, which, though more tenuous than gossamer, are stronger than steel. As a man's biography comes suitably after his death, but before the remembrance of him has faded from men's minds, so, since this comet is now buried in the darkness of space, a short history of it, with a fresh prediction of the time of its resurrection, may not be inappropriate.

"The first accurate observation, of which we have record, was made by Mr. Finlay at the Cape of Good Hope, on September 7, ten days before the perihelion passage. Just before it reached its perihelion the same observer watched its disappearance among the undulations of the sun's limb, as it began its transit, describing an apparent path not dissimilar to that of Venus on December 8, 1874, though opposite in direction. Its velocity was nearly 300 miles per second, and it dashed through the outer envelope of the sun at a distance of 280,000 miles from his surface. . . . Its nucleus then appeared as a fiery globular mass, whose diameter was about 2,000 miles. Prof. Peters observed five or six envelopes rising sunward to a height of 12,000 miles above the straw-coloured nucleus, while opposite the sun extended a brush 38,000 miles long. In a few days the nucleus appeared elongated, and soon

seemed to split into a number of parts; or, more exactly, a number of centres of condensation were formed, ranged in a right line, connected by, and enveloped in, the corona. As the weeks went by these centres changed their appearance and relative positions, and gradually became fainter, till June, 1883, when the comet was observed for the last time, at Cordoba.

"Its spectrum, which was so brilliant that it was studied in the daytime, was chiefly characterized by the presence of the bright lines of sodium; a narrow continuous spectrum given by the nucleus was clearly seen. Hence we conclude that its density was considerable, and that a collision between such a body and the earth would be fraught with disastrous results to us. It now remains to collect all the observations, and compute the orbit as accurately as possible. Here a formidable difficulty arises. Since the comet approached very near the sun, the intense heat caused a violent action in the nucleus; as the fluid matter rose toward the sun, and was again repelled, it seems that certain quite dense portions of the nucleus were carried off. After perihelion, a cooling process set in, and the denser portions drew the surrounding fluid matter to themselves; thus, the various condensations were formed. Now, different observers observed different portions of the head. . . . Observations of the relative positions of the centres of condensation were made by Law, the best series being at Athens, Cape of Good Hope, and Mt. Lookout. . . . The observations have not shown any effect of the resistance of the corona."

Since all the drawings and discussions of the nucleus have not yet been printed, the writer has made an approximation to the true orbit, by the aid of the observations made at Cordoba. The same point in the nucleus was observed until February 12, 1883; after that date the point was invisible, and the centre of the elongated nebulous mass was taken. Professor Howe arrived at the following elements:

$$
\begin{array}{llll}
T & \text{Sept. } 17{\cdot}2637, \text{ G.M.T. } 1882. & & \\
\pi & 55^{\circ}\ \ 2' & \multirow{3}{*}{$\left.\begin{array}{l}\\ \\ \\ \end{array}\right\}$ 1883{\cdot}0} & \\
\Omega & 345\ \ 44 & & \phi\ 89^{\circ}\ 14' \\
\iota & 141\ \ 55 & & \\
 & & \text{Period } 782{\cdot}4 \text{ years.} & \\
\log e & 9{\cdot}99996 & & \\
\log q & 7{\cdot}88218 & & \\
\log a & 1{\cdot}9289 & &
\end{array}
$$

The perihelion distance is 707,500 miles, the semi-major axis 7,878,000,000 miles, and the semi-minor axis 105,600,000 miles. If this be the comet which appeared in 370 B.C., whose orbit was calculated by Pingré from Greek observations, its mean period during three revolutions has been 751 years. The comet of 370 B.C. had a very small perihelion distance, and was otherwise similar to that of 1882.

Meteorological Observations made at the Adelaide Observatory, and other places in South Australia and the Northern Territory, during the year 1882, under the direction of Charles Todd, C.M.G., F.R.A.S., Observer, Postmaster-General, and Superintendent of Telegraphs. Published by authority of the Government of South Australia. Adelaide: 1885. Pp. xxxv., 298.

Besides the usual copious tables and diagrams, this volume has isobar maps, from August 1st to 5th, inclusive, copied from the maps issued at the time, exhibiting the general character of the cyclonic disturbances which successively skirt the southern shores of Australia, as the areas of

low pressure pass from west to east, an anti-cyclonic area typical of the winter months, all the while resting over the central portions of the continent. Appendix III. gives an abstract of meteorological results at 14 out stations. Appendix II. has observations on the phenomena of Jupiter's satellites, and the observations of the transit of Venus, December, 7th, 1882, by Mr. Todd, at Wentworth, at the junction of the Murray and Darling Rivers. The egress was well observed with an excellent equatorial of 4½ in., which formerly belonged to Mr. Charles Babbage, and was purchased for the observatory from his son, the late Mr. B. H. Babbage, who brought it from England after his father's death. Three diagrams show successive phases at egress: A, December 6th, 17h. 30m. 15s., Wentworth mean time, when a few very fine ligaments appeared to connect the limb of the planet with that of the sun, and the streak of light round the limb was not again continuous, although the planet was well on the disc. B, 17h. 30m. 58·6s., the ligaments, which have been gradually thickening since the last time noted, have closed up and the contact now seems tangential. C, 17h. 32m. 7s., planet evidently partly off the sun's limb. When the planet was on the disc, at some distance from contact, it was sensibly oval, and appeared of a greyish black. The longitude of the observing station, by meridian transits of 13 stars, and telegraph time signals exchanged with the Adelaide observatory, is 9h. 27m. 37·18s. east of Greenwich. The latitude, by 11 meridian altitudes with a sextant by Mr. J. W. Conolly, surveyor, is 34h. 6m. 24·7s. Mr. Todd thinks that the times of the different phases above mentioned are as exact as the nature of the observation will admit.

On the use of Carbon Bisulphide in prisms. Being an account of experiments made by the late Dr. Henry Draper, of New York. From the American Journal of Science, Vol. XXIX., April, 1885, pp. 10.

This paper has been prepared from Dr. Draper's notes by Mr. George F. Barker, who also made some supplementary measurements to test the efficiency of the apparatus. The liquid in the prism is kept in active motion by a propeller, with the result that, though extremely sensitive to variations of temperature, it gives admirable definition. Moreover, by means of a suitable thermostatic arrangement, such as an even temperature box constructed about the prism, and filled with cotton, shifting of the spectrum lines is completely obviated. When this box has been at a constant temperature for a sufficient time, the definition is good, even without the stirrer. The dispersive power of the Thollon bisulphide prism made by Hilger, of London, was equal to that of about four of the Steinheil flint prisms. Dr. Henry Draper did not live to complete this important research. This paper may, however, be of service to others engaged in photographing the prismatic spectrum. Now that the definition can be made permanently sharp, and the shifting prevented, the Thollon bisulphide prism must replace trains of glass prisms, and even gratings, unless these are of large size and are used with telescopes of proportionately large apertures. Mrs. Draper, who also took part in these experiments, lent her assistance in the compilation of this valuable article.

We have received the 39th Annual Report of the Observatory of Harvard College, 1885. Notwithstanding that it has been necessary to part with five assistants in order to reduce the expenses of the observatory, a large amount of valuable work continues to be done. Some of the items are eclipses of Jupiter's satellites, photometrically observed, revision of zone observations, standards of stellar magnitude, observations of comets shortly after discovery, when too faint to be observed with any

but powerful telescopes, spectra and colours of stars, revision of D.M. magnitudes. With the meridian circle 3,528 observations have been made, and 141 series of observations with the meridian photometer by Professor Pickering himself and Mr. Wendell. The small almucantar, devised by Mr. Chandler, and very successfully employed by him, has led to the construction of a similar instrument of large size with telescope 4 in. ap. and 43·8 in. focal length. With this instrument very satisfactory results have been obtained. The probable error of a single observation of zenith distance is in general $\pm$ 0″·37, and of a single observation of R.A. $\pm$ 0·025s. The latitude of the dome of the observatory given by three nights' work with the new almucantar is only 0″·05 less than the value found last year by the small instrument. Professor Pickering remarks, "The character of the various results given above encourages the hope that the use of the almucantar may become a permanent part of the work done at this observatory. Should the results prove no more accordant than those of other instruments, the fact that they are obtained by an entirely independent method would free them from many of the errors which are commonly repeated in meridian observations." Observations of variable stars have been continued. The second part of Vol. XIV. of the Annals of the Observatory is in course of publication. The principal columns of Part I., catalogue of 4,260 stars, with their approximate places for 1880, as well as the magnitudes determined by the photometer, and by estimates, have been reprinted, and are offered for sale at the cost of publication, under the title of Harvard Photometry. In conclusion, the report gives a list of official communications from the observatory, and of papers prepared by its officers individually.

Annales de l'Observatoire Royal de Bruxelles. Nouvelle Série. Annales Astronomiques. Tome v. Premier Fascicule. *Passage de Vénus du 6 Décembre,* 1882. Première partie. *Exposé des Résultats des Observations faites aux Stations Belges, à l'aide d'Héliomètres à Foyers inégaux.* Par. J. C. Houzeau, Directeur de l'Observatoire, Membre de l'Académie Royale de Belgique. Bruxelles : F. Hayez, 108, Rue de Louvain. 1884. pp. 35.

As far back as 1871 M. Houzeau had prepared a modified heliometer for taking a large number of micrometrical observations during the transit of Vensus. In 1882 the Government enabled him to provide both the Belgian stations with a heliometer of unequal foci. These have the two half lenses belonging to objectives of different foci, giving two unequal images, which can be superposed. If the focal distances are taken nearly in the proportion of the apparent diameters of the sun and Venus on the day of transit, the small image of the sun will be nearly equal to the large image of Venus ; leaving to the latter a slight excess. The dark spot on which the bright one is superposed will extend slightly beyond all parts of the luminous disc of the small sun. These dark and bright circles can be centred on each other with very great precision and facility. The advantages of this over the ordinary form are, first, the greater rapidity with which measures can be taken, and therefore their considerable multiplication during the transit. In fact, at Santiago of Chili, 606 measures were taken in 308 minutes. Secondly, the being able to obtain at once the distance from centre to centre of the two bodies ; a single measure without reversion sufficing. Moreover, the measures with the ordinary heliometer are affected by the faults in circularity, whether real or apparent, of the sun's disc; they depend upon only two parts of the limb, whereas in the modified heliometer the whole limb is made use of.

The heliometers were constructed by H. Grubb, of Dublin, from the drawings of M. L. Niesten. During the work Mr. Grubb suggested various improvements in detail. The images are projected on a screen, supported by a system of rigid metallic wires, beyond the eye-piece. The diameter of the image upon the screen is 6·3-in., so that each second of arc is represented by a space of about ·047-in. The precision in the centering of the small sun on Venus greatly exceeded this limit. A full description of the heliometer is accompanied by plates.

The two selected stations were: 1. Santiago du Chili, on the grounds of the National Observatory, where M. Vergara, its Director, and the authorities, afforded every assistance. This expedition was composed of L. Niesten, of the Royal Observatory of Brussels (Chief), C. Lagrange, of the same Observatory, and Captain J. Niesten, of the artillery. 2. San Antonio, Texas, where the astronomers of Washington, and especially Rear-Admiral Rowan, Director of the National Observatory, and Professor Harkness, gave the party a most obliging reception. The military authorities, with Major-General Augur at their head, rendered every service in their power. "We were so placed," says M. Houzeau, "that rather more than a radius of the terrestrial globe separated our two instruments."

The report next treats of the latitude and longitude of the stations. The assumed parallax for the preliminary calculations was $8''{\cdot}860$. The least distance of the two bodies at both stations, and for the centre of the earth, were computed, which last had to be corrected for parallax in altitude, and for refraction. The quantity X, "the critical difference," when found, would be the correction to the assumed parallax. For the ellipticity of the earth the last ellipsoid of Clarke (*Phil. Mag.*, fifth Series, Vol. VI., 1878, p. 86) was taken. The calculated value of X is $19''{\cdot}541$, the variation of which is more than double that of the parallax. The discussion of the Chili observations left 58 groups of measures resting on 594 single observations. Of these, 28 groups fell before, and 30 after the least distance. To all the groups was assigned an equal weight. At Santiago the sun was near the zenith. Personal equations were found to be very small, and within the limits of errors of observation. The weather at San Antonio was cloudy, but 126 micrometrical distances were obtained, out of which 22 groups remained, resting on 110 single observations. The least distances observed at the two stations are less than the calculated values, at Santiago by $2''{\cdot}125$, and San Antonio by $2''{\cdot}006$, which seems to indicate that the relative latitudes of the two bodies should be diminished by about $2''$ However, the accord of the two numbers is evidently of a nature to inspire confidence in the value of X, which gives an equatorial horizontal parallax of the sun, $8''{\cdot}860 + 0''{\cdot}051 = 8''{\cdot}911$, with a probable error $\mp 0''{\cdot}084$.

Many tables, formulæ, &c., appear in this report, which seems to be of great value as an independent approximation to the solution of this most delicate problem, by a new method. We may add that the above parallax corresponds to a distance, roundly, of 91,800,000 miles, with a probable error of over 900,000 miles, more or less.

We have received a paper from Ernst Grouth, M.D., *On the past and present distances of the planets from the sun.* Omitting the first part, which is quoted from the "Physical Astronomy" of Woodhouse, ch. xxiv., p. 468, proving that an increase of mass of a central body would diminish the distance and period of another revolving round it, and contrariwise, a decrease of mass would increase the distance and period of a revolving body. Dr. Grouth continues: "Such an example we have before us

with the sun and planets; for each individual planet could not have had originally the distance from the sun which it now possesses; they must have been all nearer to him than now. As Uranus was thrown off Neptune flew further away, and as Saturn was formed Uranus and Neptune again increased their distance, &c., and also our earth was twice subjected to this fate, namely, at the periods when Venus and Mercury were formed. Such was also the case with a planet X, between Jupiter and Mars, as the latter came into existence. By the adoption of this theory we shall not only be enabled to consistently account for the greater number of satellites of the superior planets, the destruction of the planet X, and the consequent formation of the great number of minor planets from its debris; but be also able to account for the two glacial periods of our earth, which, as many things show, must have come suddenly, and which, upon any other hypothesis, would remain utterly inexplicable. The late Dr. Philipp Spiller, Professor of Physics, advanced similar views in a work published in Berlin, 1871."

We think that Dr. Grouth has overlooked the enormous period that must have elapsed (on the nebular hypothesis) since the planets were formed, and also that he has much overrated the consequent diminution of the sun's mass and its effect.* The last glacial and the many *inter-glacial* periods are comparatively quite modern; nor did we ever understand that they came on *suddenly*. The writer would appear not to have seen the researches of Dr. Croll on this subject.

Annales de l'Observatoire Royal de Bruxelles. Publiées aux frais de l'Etat Nouvelle Série. Annales Astronomiques. Tome iv. Bruxelles: F. Hayez. 1883. pp. 118. A. 94. 6 c.

This volume contains observations made with the meridian telescope during the years 1879, 1880, and 1881, and the concluded right ascensions of stars, by MM. L. Goemans and E. Byl. Phenomena of the satellites of Jupiter, 1880 and 1881; and observations of the physical aspect of that planet during the oppositions of 1879, 1880, and 1881, with many illustrations. Observations of the comets *b*, 1881, and *c*, 1881, illustrated. Lunar sketches made with the 5·9-in. equatorial in 1879, 1880, and 1881, with powers generally of 180 and 270, reproduced directly from the original by the phototype process; and, lastly, studies of the solar spectrum, the first work of the kind that has been published in these annals. The lunar drawings are by E. Stuyvaert, associate astronomer. On account of the immense differences in lunar scenery occasioned by the ever varying illumination, the observations are confined to the notice of points of disagreement with the maps of MM. Schmidt and Neison. The same letters have been used as in Neison's work, "The Moon." The phenomena of Jupiter's satellites were observed by L. Niesten.

Boletin de la Institucion libre de Ensenanza. Ano IX., Madrid 31 de Enero de 1885. Num. 121.

Under the head of El Observatorio de la Institucion, Seccion Meteorologica, is a paper by D. Augusto Arcimis on meteorological instruments and observations. As far as we can make it out, not having studied Spanish, it seems of a popular and useful character. It is to be continued.

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* We fancy that, for example, if a mass equal to that of Jupiter were to be taken from the sun, the effect would be to increase the radius vector of the earth only by about 45,000 miles.

Observations of Variable Stars in 1824. By Edward C. Pickering. Communicated March 11th, 1885.*

Professor Pickering says, "The friendly co-operation of several astronomers interested in the subject makes it practicable to present on this occasion a much fuller view of the progress of observation, in Europe as well as in America, than could be given last year." We find that 13 observers in America, this country, and the continent, have contributed to these observations, which embrace 143 stars. In the conclusion of this interesting paper, Professor Pickering expresses the hope that observers of variable stars will continue to furnish accounts of their work during each year as soon as possible after its close. It is desirable that these accounts should be received at the Harvard College Observatory as early as February 1st of the following year.

THE PLANETS FOR NOVEMBER.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	15 5 41	S. 18 24½	4″·8	0 21·9
	9th	15 55 15	S. 22 6½	5″·0	0 39·9
	17th	16 45 9	S. 24 38	5″·4	0 58·2
	25th	17 33 2	S. 25 47	6″·0	1 14·5
Venus ...	1st	17 30 15	S. 25 50	17″·6	2 46·1
	9th	18 10 58	S. 27 12	18″·6	2 55·2
	17th	18 51 1	S. 25 49	20″·0	3 3·7
	25th	19 29 40	S. 24 43½	21″·6	3 10·8
Mars ...	1st	9 58 8	N. 14 10	7″·0	19 11·2
	9th	10 14 20	N. 12 50½	7″·2	18 55·9
	17th	10 29 42	N. 11 32½	7″·6	18 39·8
	25th	10 44 10	N. 10 16½	8″·0	18 22·8
Jupiter ...	21st	12 5 32	N. 0 39½	31″·4	19 59·7
	29th	12 9 53	N. 0 13	32″·0	19 32·6
Saturn ...	1st	6 35 41	N. 22 17½	17″·6	15 49·3
	9th	6 34 39	N. 22 18½	17″·8	15 16·9
	17th	6 24 17	N. 22 28	18″·6	14 43·9
	25th	6 21 28	N. 22 30	18″·6	14 10·5
Neptune ...	1st	3 30 40	N. 17 11		12 44·8
	17th	3 28 51	N. 17 4½		11 40·1

Mercury sets about a quarter of an hour after the sun on the 1st, the interval increasing.

Venus sets about an hour and three quarters after the sun on the 1st, the interval increasing.

Mars rises a few minutes before midnight at the beginning of the month, the interval increasing.

Jupiter rises nearly three hours after midnight on the 1st, the interval decreasing.

Saturn rises about three hours after sunset, and afterwards earlier each night.

* Proc. Amer. Acad. Arts and Sciences.

ASTRONOMICAL OCCURRENCES FOR NOVEMBER, 1885.

DATE.		Principal Occurrences. h. m.		Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m.
Sun	1		Sidereal Time at Mean Noon 14h. 43m. 44·79s.			*α* Persei. 12 30·4
Mon	2	21	Conjunction of Moon and Jupiter 0° 52′ N.			12 26·5
Tues	3		Sun's Meridian Passage 16m. 9·52s. before Mean Noon			12 22·5
Wed	4		Saturn's Ring: Major axis=44″·48 Minor axis=19″·09	1st Ec. D.	17 1 12	12 18·6
Thur	5			1st Tr. I. 1st Sh. E. 1st Tr. E. 4th Sh. E.	15 0 16 25 17 18 17 43	12 14·7
Fri	6	9 2	● New Moon	1st Oc. R.	14 39	12 10·7
Sat	7	9	Conjunction of Moon and Mercury 6° 16′ S.	2nd Ec. D. 2nd Oc. R.	16 54 35 21 32	12 6·8
Sun	8			3rd Oc. R.	17 40	12 2·9
Mon	9			2nd Sh. E. 2nd Tr. E.	14 51 16 42	11 58·9
Tues	10	8	Conjunction of Moon and Venus 7° 49′ S.			11 55·0
Wed	11			1st Ec. D.	12 54 42	11 51·1
Thur	12			1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	16 0 16 59 18 18 19 16	11 47·1
Fri	13			1st Oc. R.	16 37	11 43·2
Sat	14	9 59	☽ Moon's First Quarter	4th Oc. R.	17 0	11 39·3
Sun	15	20	Neptune at opposition to the Sun Illuminated portion of disc of Venus=0·608 Illuminated portion of disc of Mars=0·901	3rd Ec. D. 3rd Ec. R. 3rd Oc. D.	14 45 35 17 51 5 18 47	11 35·3
Mon	16		Sidereal Time at Mean Noon 15h. 46m. 49·66s.	2nd Sh. I. 2nd Tr. I. 2nd Sh. E.	14 33 16 35 17 25	11 31·4
Tues	17	10 41 11 44	Occultation of B.A.C. 8365 (6½) Reappearance of ditto			11 27·5
Wed	18		Sun's Meridian Passage 14m. 35·96s. before Mean Noon			11 23·5

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Persei.
Thur	19			1st Sh. I. 1st Tr. I.	17 54 18 56	11 19·6
Fri	20			1st Ec. D. 1st Oc. R.	15 16 30 18 34	11 15·7
Sat	21	21 39	○ Full Moon	1st Sh. E. 1st Tr. E.	14 39 15 42	11 11·7
Sun	22	6 29 7 17 6 38 7 9 6 49 7 8 7 18 8 17 9 48 10 57	Occultation of θ^1 Tauri (4½) Reappearance of ditto Occultation of θ^2 Tauri (4½) Reappearance of ditto Occultation of 75 Tauri (6) Reappearance of ditto Occultation of B.A.C. 1391 (5) Reappearance of ditto Occultation of α Tauri (1) Reappearance of ditto	4th Sh. E. 3rd Ec. D.	15 2 18 43 11	11 7·8
Mon	23	5 59 6 42 14 52 15 58	Occultation of 117 Tauri (6) Reappearance of ditto Occultation of 130 Tauri (6) Reappearance of ditto	1st Sh. I. 1st Tr. I.	17 8 19 18	11 3·9
Tues	24	10 59 12 6 11	Occultation of 26 Geminorum (5½) Reappearance of ditto Conjunction of Moon and Saturn 3° 59′ N. Saturn's Ring: Major axis=45″·75 Minor axis=19″·76			10 59·9
Wed	25	17 40 18 46 11 18	Occultation of 1 Cancri (6) Reappearance of ditto Occultation reappearance of 48 Leonis (5)	2nd Oc. R.	16 19	10 56·0
Thur	26			3rd Tr. E.	16 2	10 52·1
Fri	27			1st Ec. D.	17 9 48	10 48·1
Sat	28	13 57 21	☾ Moon's Last Quarter Conjunction of Moon and Mars 3° 23′ N.	1st Sh. I.	14 15	10 44·2
Sun	29			1st Oc. R.	15 0	10 40·3
Mon	30	17 0 18 9 11	Occultation of URANUS Reappearance of ditto Conjunction of Moon and Jupiter 0° 29′ N.	2nd Sh. I.	19 42	10 36·3

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
Nov. 1	−25 46 E.	Nov. 11	+32 32 M.	Nov. 21	−89 9 M.
2	37 55	12	20 21	22	+79 42 E.
3	50 6	13	8 12	23	66 31
4	62 16	14	− 3 59	24	54 22
5	74 27	15	16 10	25	42 12
6	86 37	16	28 19	26	30 1
7	+81 13 M.	17	40 30	27	17 52
8	69 2	18	52 39	28	5 42
9	56 52	19	64 49	29	− 6 27
10	44 41	20	76 59	30	18 37

Moon farthest from the Earth, Nov. 12, 14h.
,, nearest the ,, ,, 24, 9h.

Books received :—Bulletin Astronomique.—Nouvelles Recherches sur L'Agrandissement apparent des Constellations, &c., à l'Horizon. Par M. Paul Stroobant. Bruxelles. F. Harag. 1885.—Intimate connexion between Gravitation and the Solar Parallax. By Thos. Bassnett. Jacksonville.—Astronomische Nachrichten.—Sirius.—L'Astronomie. Par M. Flammarion.—Sidereal Messenger. Northfield, Minna.—The Lich Observatory, California. By D. P. Todd.—Third Report of the Committee of Stellar Magnitudes.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

August, 1885.
Esdaile, J. E.

To Dec., 1885.
Green, G. W.
Matthews, Rev. W.
Murgatroyd, J.
Ward, J. M.

To Jan., 1886.
Daw, E.

To June, 1886.
Gooch, Miss.
Backhouse, T. W.

TO CORRESPONDENTS.

All communications of any kind should be addressed to the Editor, 11, Angel Court, Throgmorton Street, London, E.C.

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The Astronomical Register.

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ROYAL ASTRONOMICAL SOCIETY.

Session 1885—86.

The first meeting after the long vacation was held at the Society's Rooms, Burlington House, on the evening of Friday, November 13th, at eight o'clock.

Ed. Dunkin, Esq., F.R.S., *President*, in the Chair.

Secretaries—E. B. Knobel, Esq., and Lieut.-Col. Tupman.

The Rev. Stephen H. Saxby, of East Clevedon, Somerset, was balloted for, and duly elected a Fellow of the Society.

Colonel Tupman announced that 200 presents had been received since the last meeting. Amongst them was a photograph of part of the milky way, which deserved special mention. It had been taken by the Brothers Henry, of the Paris Observatory, with an exposure of an hour. The star discs were perfectly round and sharp, showing that the driving of the telescope during the exposure was practically perfect, and that the object glass had been corrected with great nicety for the photographic rays.

Mr. Knobel read a paper by Mr. C. R. Marten, entitled "Notes on the Total Solar Eclipse of 1885, September 9, viewed from The Terrace, Wellington, New Zealand." The conditions of the weather at first were not promising; but after 7.4 a.m. there were no impediments to the observations, which were made at 100 feet above the level of the sea. An ordinary binocular field-glass was used, with an excellent chronograph and registering thermometers. At 7.4 a.m. the sun appeared through a rift in the cloud, and after that an absolutely perfect view of the eclipse

was obtained. No noteworthy diminution of light was observed until about two-thirds of the solar disc had been covered, and then the light faded away swiftly. As totality approached, the shadows were weird and striking. About 15 or 20 seconds before the entire disappearance of the sun, the entire disc of the moon became suddenly visible against the white background of the solar corona. An instant before totality, the solar light was centred into one intensely brilliant point, which shone like an electric arc, and then came complete obscuration. The darkness resembled that of a moonlight night. This he attributed to bulky masses of cloud on the horizon, which shone with the tints of sunset and sunrise, which have been observed since the Krakatoa explosion. The Corona shone with a soft light, and extended to a distance of two lunar diameters or more from the moon's limb. Another long ray proceeded from the lower right quadrant, and there was a large equatorial protuberance resembling the Aurora Australis, and there was an unsteadiness and a varying degree of luminosity in the coronal rays which reminded him of the auroral light. Just before the re-appearance of the sun red prominences were visible, with remarkable brilliancy of colour, varying from pink and rose to scarlet and crimson. These could be seen with the naked eye. There was a fall of temperature of 5·5° during the eclipse, and the disturbed state of the atmosphere might perhaps be responsible for some variation and unsteadiness in the coronal light and red flames.

Mr. Ranyard said: I don't think it is necessary to assume that the colours on the horizon were caused by dust from Krakatoa. Such colours have been very frequently observed during other eclipses; in fact, nearly every observer of a total eclipse is struck by them. There are more observations of these colours than of any other phenomena associated with eclipses. The colours seem to be sufficiently explained when we take into account that the landscape and clouds just outside the area of totality are lit up by the light from the thin solar crescent. This is the chief source of illumination within the shadow area, and the light has to travel through a long distance in the lower atmosphere before it reaches the observer, so that the blue or short wave lengths of the spectrum are sieved out first, as in the morning and evening illumination. The effect produced is very weird.

Mr. Knobel: Is Mr. Ranyard aware of any observations of the unsteadiness of the corona, such as is described by Mr. Marten? He says there was some unsteadiness and a distinctly varying degree of luminosity in the coronal rays.

Mr. Ranyard: Yes. Such observations are not at all un-

common. During the totality the atmosphere is often very disturbed; "an eclipse wind" springs up as totality comes on, and in other cases where there have been gusts of wind the air suddenly becomes calm; probably the air within the shadow cone is a little cooler than the surrounding air, and sinks, causing outward currents along the earth's surface.

Mr. Penrose: In observing the eclipse of 1878, I thought I noticed this unsteadiness. There were some very definite long filaments near to the south pole of the sun, and I had a decided impression of having seen the ends of these filaments tremble.

General Tennant: Does Mr. Marten mention anything about those waves of shadow which many observers have seen pass across the fields? They sometimes have been called colour waves. I think that General Addison, at Aden, in 1868, saw waves, or bands of light and shadow passing across the walls. But where do these bands come from? They appear near the time of totality, but one cannot understand them.

Mr. Ranyard: There is plenty of evidence that they do exist; they travel more or less slowly over the ground, not always in the direction in which the wind is blowing, and their direction does not correspond with the direction of the motion of the moon's shadow. I think that they may be accounted for by the fact that nearly all the light within the area of totality is derived from the coloured clouds upon the horizon. Some persons have attempted to observe the shadow cast by the corona during totality, but they have found that a pencil or other object held up casts no shadow; in fact, the light from the corona vanishes in comparison with the illumination derived from the horizon, which is so bright that, looking towards the horizon, near objects appear black on a bright background. It is easy to see how light from the horizon coming slantingly along the surface of the earth would be interfered with by the variations of density in the atmosphere caused by the gusts of totality wind.

General Tennant: But General Addison saw them on the walls.

Mr. Ranyard: They would be better seen on the walls than on the ground, because, if the light comes from the horizon they would be received perpendicularly.

General Tennant: But you cannot have rays of light form dark bands. If you were to attribute it to a nervous feeling in the observer's eye on the change of light, I could quite understand it; but I find it difficult to understand it as a real phenomena.

Mr. Penrose: I have seen the shadows running on the ground, but it was after the reappearance of the sun.

Mr. Stone: When the sun was exerting its power?

Mr. Penrose: Yes; just after totality was over.

Mr. Klein: Might not this flickering be due to the fact that the last part of the sun which is seen is a mere point of light, so that the shadows are sharper, as with the electric light?

Mr. Ranyard: I do not think that can be the cause; there is really a very large surface of cloud and atmospheric haze lit up, from which the light within the shadow area is derived. When totality commences the source of light is on one side towards the middle of totality the horizon is lit up in all directions, and towards the end of totality it is derived from the other side. I do not think that the phenomena can arise from any nervous effect, because different people have observed the bands at the same time, and concur in their estimates as to the breadth of the bands, their direction, rate of motion, and the direction in which they travel.

Mr. Stone: In the only eclipses I have seen the air was excessively still, and I saw no tremors, which have been spoken of. During the time I saw the corona there was no change, and no indications of tremor; and there were no clouds in the neighbourhood. I think that some of these disturbances are entirely atmospheric; at the same time there is a marked appearance of change in the aspect of the country as totality is going on, and there was a nervous creeping came over everybody. I can understand that if imagination was allowed play observers might see anything. Certainly no change took place in the corona.

General Tennant: I cannot conceive it to be imagination; but there is the fact. How it is to be accounted for I cannot say.

The President: So far as my experience goes in observing the total eclipse of 1851, I noticed no tremor whatever in the telescope during the time of totality. The images of the sun, the prominences, and corona, were perfectly steady. With regard to the shadow, it was seen coming from the west horizon, and travelling on and on towards the east; and that shadow all my assistants noticed. While my eye was at the telescope one of these gentlemen said, "I can see the shadow distinctly coming"; and afterwards I saw it myself as it went off. We are very much obliged to Mr. Marten for communicating these observations, as we may perhaps have no other notices of the eclipse from that part of the world. (Hear, hear.)

Mr. Knobel read a paper by Professor Pritchard, *On photometric observations of the nova in the Andromeda nebula, observed at Oxford.* The paper described the observations made for measuring the brightness of the new star in Andromeda. It was first observed on Sept. 7, when its brightness was measured at 8·25,

The star declined till the end of September, when it was about the 10th magnitude, at which magnitude it has continued till November 12. But there was considerable difficulty in observing the brightness of this new star, on account of the brightness of the nebula in the background. Owing to the small area of the wedge there was, near the point of extinction, some trace of unextinguished light from the nebula. From some cause or other the behaviour of the star in the nebula, as it approached extinction, was somewhat different to an ordinary star, but in the large refractor there was no perceptible difference between the nova and other stars of similar brightness. The table ended with the magnitudes from Sept. 7, when it was 8·25, to Oct. 16, when it was 9·96.

Mr. Knobel read a note by Father Perry, *On the outburst in Andromeda.* The nebula had been observed at Stonyhurst from Sept. 13 to Nov. 8. On Sept. 13 the spectrum was continuous at the red end, and there was a decided maximum of brightness in the Green. A bright band in the Green was suspected, but not clearly seen. On Oct. 9 the spectrum was still brightest in the green. At present, though it was decidedly stellar—as was made evident by increase of power—there was no great change from day to day, the magnitude remaining about 11. With a low power the nucleus alone was visible. With powers of 200 and 300 the nova is more brilliant than the nebula.

Mr. Maunder read a paper *On the spectrum of the new star in the great nebula in Andromeda, made at the Royal Observatory, Greenwich.* On September 4 he used a half-prism spectroscope, and was not able to see any lines, dark or bright, but simply a continuous spectrum from D to F. Probably the dispersion was a little too high. But on Sept. 11, using a single-prism spectroscope, he was able to see what appeared to be two bright lines, and he got a rough measure of their places. On the same night he examined two stars near the nova, in order to see whether the great faintness at the red end was the same in other stars. It appeared that the red end was considerably fainter in the nova than in an ordinary star in the neighbourhood. On Sept. 15 he used simply a prism before the eye-piece, and again two bright points were suspected. On Sept. 30, with a half-prism spectroscope reversed, he saw the lines more distinctly than before, and believed he made out a third line. One of the lines he fancied he was able to trace into the nebula, but it was rather difficult to keep the star steady on the slit, and he could not quite make sure of that point. Mr. Maunder then gave the wave-lengths of the lines on the black board, and added that the spectrum of the nebula was traced nearly as far as the

violet line of hydrogen, and the spectrum of the nova a little farther, probably owing to its greater brightness; but at the red end the spectrum of the nova was not traceable nearly as far as in other stars of the same magnitude, and the spectrum of the nebula was not traceable nearly so far at the red end as it could be traced in the spectrum of the nova. The brightest parts were between D and F. The spectrum of the nova began nearly at C, and was lost a little beyond G, and the brightest parts were from D to F.

Mr. Turner: I am afraid I can only repeat a remark second-hand. Professor Pritchard communicated his magnitudes to us at Greenwich, and the general impression was that the star was brighter than his measures gave it. That seemed to be the general impression of all the observers at Greenwich as regards the magnitude of the star. I do not known whether we have any other photometric estimates of the magnitude.

Mr. Knobel: I have made some photometric observations of this star, and I quite agree with Mr. Turner that the photometric measures that have been made probably make the star fainter than it appeared to the eye. I have made observations from September 3rd up to last Saturday, when I last saw it. I made 12 or 14 extinctions on each night that I had the chance of seeing it, and I have come to the conclusion that all the extinctions I have made on the principle of limited apertures make the star too faint. I believe that the objection to the observations I made, which give the star too faint, applies also to those made by the wedge photometer. The star is extinguished when it becomes invisible to the eye, and it becomes invisible to the eye when it cannot be distinguished from the nebula which forms the background; and the aperture which makes it invisible to me in every case seems to be much too large, and therefore makes the star much too faint, according to the comparison star which has been used, so that I find a systematic error which runs through all my observations, and makes the star fainter than it really appears to the naked eye. I disagree with Professor Pritchard in one statement he makes; he says that "about the end of September this star approached closely to the 10th magnitude, to which it has continued constant to the present time." I do not think that is so. I observed it carefully last Saturday (November 7th)—cloudy weather had prevented my observing it at all since November 1st. On November 1st I carefully compared it with the small star that preceded the nebula by 112 or 114 seconds, and on November 1st it was three-fourths of a magnitude brighter than that star. On November 7th it had unquestionably decreased in brightness

as compared with what it was on the 1st, and it was, in my estimation, about half a magnitude brighter than the small star which precedes it by 112 seconds.

Mr. Stone: About the 12th magnitude?

Mr. Knobel: Yes; and the nova was less bright on the 7th than it was on the 1st by about $\frac{1}{4}$ of a magnitude. I would like to ask Mr. Maunder whether his observations on Sept. 4th were made with a spectroscope of rather too high a dispersion; because when Dr. Huggins made his observations on Sept. 3rd he found that with too high a dispersion he was unable to see any indication of bright lines; but by using a spectroscope with a lower dispersion he was able to see indications of bright lines.

Mr. Stone: Does Dr. Huggins say unquestionably? Was it not rather that he suspected the bright lines?

Mr. Knobel: No; I had a letter recently, and his expression was stronger than that. He was satisfied.*

Mr. Stone: The only reason I ask is this: I think, from the observations of the spectrum of τ Corona we made at Greenwich, there should be no doubt about the linear spectrum. I am judging from observations we made of the linear spectrum of τ Corona until it was below the 8th magnitude. When it got much below the 8th magnitude then it was a doubtful thing to say whether the spectrum was linear or not.

Mr. Knobel: The star was very much brighter than that.

Mr. Stone: I watched the linear spectrum of τ Corona fading until it was considerably below the 8th magnitude, and I could still distinguish the character of the spectrum below that.

Mr. Knott: Perhaps I might be allowed to make a remark on the question of the magnitude of the star. I had no opportunity of observing it until the end of September; but on the 28th Sept. I had a very short view of it. The clouds came up almost immediately, so I had only the opportunity of seeing it for a few minutes, but I thought it was somewhere about the $8\frac{1}{2}$ or 9th magnitude. But that estimate must have been too high. It was made in a very short time. On Oct. 1st, when I saw it next, I thought it seemed to be about the 10th or $10\frac{1}{2}$th magnitude. Then its colour seemed whitish, and the disc of the star looked rather ill-defined—rather large for its magnitude. On 3rd Oct. I made an observation, and on Oct. 27th I gauged it by the method of limiting apertures, by diaphragms placed in front of the object-glass of my 7-inch equatorial. On Oct. 27th I gauged it about the 11th magnitude. On that night I gauged also the star which Mr. Knobel has referred to, D'Arrest's star, which precedes it, and which he called the 11·12th magnitude. That

* This expression referred to observations made on September 9.

star on that night I gauged as 12·4 mag., and then there appeared to be a difference in the magnitude of 1·4 between the nova and D'Arrest's star. On Nov. 7th the nova came out 11·8, and the magnitude of D'Arrest's star as 12·3. This last observation was made on the same night when Mr. Knobel was making his observation, and the difference between the two —½ a magnitude—is exactly the same between us; but I felt great difficulty in regard to properly gauging the magnitude, for the reason that Mr. Knobel has stated—the brightness of the background on which the star was projected. I made my gaugings with a power of 191. Thus my observations agree with Mr. Knobel's in the fact that the star decreased in magnitude considerably between October and November, which is hardly in accordance with Professor Pritchard's views.

Mr. Turner: I may mention that there is a general impression with those who use the transit circle at Greenwich that the nova was decreasing rapidly throughout October. The observations become much more difficult towards the end of the month. With regard to Mr. Knobel's remark about the photometric observations as opposed to eye observations, it appears to me we are returning to the question of the moonlight background, as to whether the background does or does not interfere with the wedge photometry.

Mr. Brett: I would like to call attention to the unreliable character of wedge photometry. Most observers cannot be depended upon below the eighth degree of magnitude. I know many good observers who will see a ninth magnitude star go out when my eye can see it long afterwards. That is perfectly true. It is a subjective matter. The eyes of some persons are excellent for high illumination, and the eyes of other people are good for low illumination; and there are many people who cannot estimate degrees below the eighth magnitude at all.

Mr. Common: I don't know whether it is worth while mentioning what I saw about this star. As regards the relative brightness of the nova and nucleus of the nebula. I thought, at first, it was at the centre of the nucleus; with a higher power I saw the distinction between the two; looking at it with the higher power, there was a marked distinction; using limited apertures and a low power, the two disappeared together; but with a high power the star remained visible long after the nebula had ceased to be visible with reduced aperture. That held good for two or three days. The first night I saw the nova it was as bright as one of the other stars, but not so bright as the other. In those stars the light is pretty constant. During the time I have watched the nova, it has gone down very rapidly in

magnitude. With regard to the question whether this new outburst is a star that existed before : I gave, in *Nature*, a copy of a photograph I took in August 1884. It embraced an area of 25 minutes all round, and shows no star whatever where the nova appears, though there are stars all over the nebula, but not in that exact spot. That is not one that my photograph reached.

Mr. Knobel : Is it not a fact that if you increase the magnifying power you can see fainter stars ?

Mr. Common : What is the cause ?

Mr. Knobel : I do not attempt to explain how. I ask, is it a fact ?

Mr. Common : It is certainly a fact.

Mr. Ranyard : It is evidently due to the decrease in the illumination of the atmosphere. If you double the magnifying power you have an area apparently four times as great of sky, so that the illumination of the background on which the stars are seen is only one-fourth what it was before, but the light of the stars is not diminished. With regard to the brightness of the lines in the spectrum of the nova I think they must have been very faint, for, when I saw the nova for the first time on the 4th of September, I did not detect them with a five-prism direct vision spectroscope, and with another of slightly less dispersion I was not able to satisfy myself with regard to their existence.

Mr. Maunder : The bright lines were certainly very faint as compared with the linear spectrum. They were only just distinguishable on it. On September 4th I think my failure to see them might possibly be due not only to the fact that I used a higher dispersion than on September 11th, but also that I did not close the slit so much. I used a narrow slit on the 11th and 30th September. I tried a cylindrical lens on September 4th, and I think that also may have had something to do with my failure on that occasion, as I saw the lines when the star was fainter; or it might be because the relative brightness of the bright lines and the continuous spectrum changed from one date to another. Of course we cannot assume that the relation of the bright line spectrum to the continuous spectrum was always the same, either in this star or in the star T Coronæ.

A vote of thanks was then passed to the authors of the papers read.

Mr. Hilger said that he had a few remarks to make on a new form of governor for driving clocks of equatorials, which, he thought, would prove of interest to those who were engaged in the photography of the heavenly bodies. In former years he had made a great number of governors, with the construction of

which many present would be acquainted, and which many years ago were considered, as perhaps they were still, especially in France, the best form of driving clocks for an equatorial. As far as the fans were concerned his governor resembled some of those made in France by Foucault. There were points in the Foucault governor with which he was by no means satisfied, especially the great complication of pieces, and the great amount of friction, which all meant tendency to error. He did not believe in any form of control which depended upon friction; and finding that, generally speaking, all driving clocks were so controlled, he had worked out the present design, which was the result of years of experience. He did not think that any governor could have less friction while in action than this particular form which he claimed as his own invention. The entire amount of friction was confined to four fine points which carried the two fans. There was no other friction in the governing action. In all other forms of governors the clock went slower either when the governor balls were closed, or when the driving-weight was reduced. It was not so with this form of clock. An additional 140 lbs., more or less, of driving-weight would make no appreciable difference in the speed. The speed remained unchanged whether the fans were wide open or nearly closed. He said nearly closed, because as soon as the fans touched the spindle the control ceased. The fans were pulled towards the spindle by chronometer springs, which would only allow them to open when a speed varying from 25 to 30 revolutions per second was obtained. The spring was only wound up half a revolution, so that the mean motion of the fans, which does not exceed 30 degrees on either side, kept the spring perfectly free in the air. There was also another very important fact in regard to this governor. There was no parallelogram motion to keep the fans inclined at equal angles to the spindle, and yet under every variation of position, whether perpendicular or horizontal, the fans always opened symmetrically. When he first tried fans he found they worked at different speeds when close to the axis, than when they were more open. That difficulty he overcame in a manner he proceeded to explain. He would be most happy to show the working of the clock, and explain any further details to any one in the room who desired further information.

Mr. Ranyard: There is one objection to this clock. The wire rope, I see, winds upon itself, and when it comes to the end of the coil there is a different diameter of driving cylinder, which would certainly make a difference in the driving power of the clock; and it also has a great tendency to wear out the wire rope,

and might cause an accident. With regard to the rubbing governor which Mr. Hilger objects to, I have no objection to a rubbing governor, because there are a great many means of controlling its rate, at least, in the form I use, which is the same form as that used by Mr. Common. My chief difficulties have arisen from the unequalities of the larger driving wheels, in which it is very difficult to get the teeth exactly equal. That is the great difficulty at present.

A vote of thanks was then passed to Mr. Hilger.

Mr. Hopkins read a paper on "Erratic Meteors." On more than one occasion he had observed that the paths of meteors were not perfectly straight; sometimes they appeared to be distinctly curved, and on two occasions he had seen meteors, whose paths appeared broken at an angle, so that their paths might be described as zig-zag. Mr. Hopkins drew upon the blackboard an illustration of the paths he had observed.

Mr. Penrose said: These zig-zag paths are analogous to the course of a thin piece of slate thrown into the air, or to the path of a boomerang, which, owing to its peculiar shape and the resistance of the air, comes back again to the hand of the thrower.

Mr. Ranyard: I think there can be no doubt about the existence of curved paths; I have seen a comparatively bright meteor which left a trail that was distinctly curved. There was a paper, on the subject, published some ten or fifteen years ago by Prof. Newton, of America. He accounted for the curvilinear paths by the irregular shape of the fragments which come into the earth's atmosphere.

The President: Many of you have seen one of the French star charts passed round the room, and you will see what a very interesting result has been obtained in Paris, and I may say also by Mr. Common. M. Mouchez considers that experiments having arrived at such a forward state, it is important that some action should now be taken to have some regular course of observations made in different parts of the world, at different observatories, so as to take photographs of the whole of the heavens. He has forwarded to me a letter which I think you would understand better in French than you would a mere translation.

The President then read the letter, from which it appeared that M. Mouchez is anxious that six or eight observatories in different parts of the world should combine and make these photographic observations on some similar principle. Six thousand photographs, each representing 7° superficial, will be necessary to make a complete survey and map of the northern and southern heavens on a scale similar to that on which the

Brothers Henry, of the Paris Observatory, are mapping the Zodiacal region.

The President here announced that the meeting would now proceed to ballot.

Mr. Common: What about the discussion on this clock?

Perhaps Mr. Hilger will explain it a little further. He stated that this is the best clock in the world. With regard to what has been said about the photography of the heavens, it all depends upon a good clock; the clock is the foundation of everything. Therefore I think it will be interesting to know all about this clock. I have very strong doubts as to whether this clock would drive in the way Mr. Hilger says it would. I believe rather in a friction governor myself.

Mr. Turner: Through the kindness of Mr. Hilger I was allowed to see the clock at his workshop, and on one occasion he put it through a series of tests. One test he subjected it to was the constancy of the driving with different weights. The clock was driven with weights varying from 120 lbs. to 240 lbs., and the constancy of rate remained practically the same. It was tested by a counter, which revolved once in a minute and a half. Originally the clock was found to drive more slowly with the heavy weight. Then Mr. Hilger adjusted the small weights, which he claims to be the adjusting feature of the clock, and showed that it could be made to drive quicker when the weight was off, and that was done to the extent of 4/10ths or 5/10ths of a second in one and a half minute. You could not, of course, make it drive quicker when all the weight was taken off; but for these two points it was practically found that the rate of the clock could be over-corrected, and there is thus a strong presumption in favour of the possibility of a practically constant rate for the intermediate range.

Mr. Penrose: Was it doing any work?

Mr. Turner: No; but the driving part of the clock was itself varied.

Mr. Common: I would like to remark that the working of these clocks, however well you may regulate them, depends entirely upon the strength of the spring, and that the spring is affected by the temperature. The temperature is an all important consideration. I have adjusted the clock I used with my three feet telescope to almost absolute accuracy. I am sure the rate was regular enough, but I found, as the night grew colder, so the clock began to gain, and the colder it got the more it gained; and here we have no compensation for temperature at all. There is no doubt at all that a good clock is of the greatest importance, especially to people engaged in photography; and I

have thought of a great many ways, and made a number of experiments as to temperature corrections, and I do not think Mr. Hilger is working upon the right lines in having a spring which is liable to change the governing power owing to the temperature.

Mr. Hilger: I have tried the clock under different temperatures for that very reason, and when the fans are wide open it makes no difference if the temperature varies, because the fans will come closer together; but the speed will keep the same whether they are wide apart or near.

The Astronomer-Royal: It seems to me that the important point about this clock is that when you take the weight off, you can make the clock go faster. I should like to ask Mr. Common if he has seen any clock in which that could be effected. With an increase in friction, the clock may be made to go faster, or it may be made to go slower, and so it may be made to go at the same rate whatever the friction or weight may be. The difficulty I have seen in the case of other clocks is that the rate varies with the load the clock has to drive.

Mr. Common: No; because nobody would think it worth while to do it. You do not want a clock to go faster if you take the weight off.

The Astronomer-Royal: You want to have the power to make it go faster as well as to go slower, then you have the power of making it go at the same speed.

Mr. Common: So you can if you can make it go slower. The only objection I take to this is that it is easy enough to make a clock now with a compensating pendulum. I say that where you have a spring which is acted upon by the temperature your error will be very much greater than with a pendulum, and hence I say that the best line to go upon in making a driving clock is to go upon the same lines as Sir George Airy went at Greenwich, namely, a circular pendulum. This clock with a spring will go very well if it is kept in a mean temperature room; but if you apply it to the rough test of actual work you will find you want compensation.

The following papers were announced, and taken as read:—

T. Mackenzie: *On the Application of a Nicol's Prism to Sextant Observations.*

J. Maguire: *Total Solar Eclipses,* 878 *to* 1724.

Capt. W. Noble: *Daylight Occultation of Aldebaran on July 9th,* 1885.

A. A. Rambaut: *To reduce the precessions computed with Bessel's constants to an agreement with those of Struve.*

Rev. T. E. Espin: *Note on Mr. Chambers' suspected new variable in Corona Borealis.*

A, Marth: *Ephemeris for Physical Observations of Mars,* 1886.

SELENOGRAPHICAL NOTES, DECEMBER, 1885.

By Thos. Gwyn Elger, F.R.A.S.

Timocharis. At 6h. on 15th November, the eastern wall of this ring plain being on the morning terminator, and the interior full of shadow, I remarked a large round black spot near the summit of the western wall, and a conspicuous oval-shaped depression slightly to the south of it, and somewhat further from the edge. The former is probably analogous to the dark spots on the southern wall of Copernicus, though, unlike these, it is not visible under a high light. Neither Neison nor Schmidt represent these features, though they are certainly prominent. North-west of Timocharis, and near the foot of the wall, is a large dark crater, or rather crater-cone, which casts a very pronounced shadow. It is connected with the wall by a ridge or spur. Schmidt represents this object as a small crater, while Mädler and Neison omit it. The wide gap, or break, in the north wall is very noteworthy at this stage of illumination, as is also the curious arrangement of the little ridges and minor inequalities of the surface immediately north of the opening. They all appear to radiate from the latter in the form of curved lines or corrugations convex to the east. About 40 miles north of Timocharis the great ridge which runs up to the wall of Lambert is at this phase a most striking object, bordered as it then is by a wide sharp band of shadow. It is, as regards its extent, one of the most remarkable ridges on the moon's visible surface. It can be traced from the north-west wall of Lambert in an unbroken line to Kirch, and then onwards in a less continuous state almost to the Teneriffe mountains south-east of Plato. At full moon, scarcely a trace of this object can be seen.

Between 7h. and 8h. on 17th November, Timocharis was examined with a power of 340, definition being very good. At this time the east longitude of the morning terminator was about 39°. The well-marked terraces of the inner slopes of the eastern and western walls were very distinctly displayed, as was also the oval-shaped central mountain with the large crater near its eastern end. This object, though discovered more than forty years ago by Schmidt, seems to have been entirely overlooked by other Selenographers. His records stand as follows :—

1842. August 28th.—The central crater of Timocharis was seen with the six-feet refractor.

1849. February 3rd.—The interior of Timocharis is very uneven. The central mountain a crater.

1849. March 4th.—There is certainly a crater in the middle of Timocharis.

1861. June 18th.—The central crater of Timocharis noted as distinctly visible.

The evident shallowness of this object probably accounts for its having escaped notice. Were it only a little deeper it would, by reason of its size, be very conspicuous, for it is at least 2½ miles in diameter, and its eastern rim is very brilliant. Mädler and Neison omit a bright crater some distance to the west of Timocharis *a*, the former showing a little mountain in its place. The ray-system of Timocharis, though among the earliest recorded by Selenographers, is not striking. It is principally developed on the western side of the formation, where it is mainly represented by three bright, though short, streaks, running in a westerly direction from the wall; one of them passing slightly to the south of Timocharis *b*, and an ill-defined glow extending for some distance on the western side of the ring-plain. There is also a faint streak directed towards Lambert from the eastern wall. At and about full moon there is a conspicuous ray running in a northerly direction from the west wall through the crater *b*.

Under a high light the interior of Timocharis is not uniform in tone. That portion of the floor lying south of the central crater is decidedly darker than the remainder, which is of the same shade as the duskier portions of the surrounding Mare.

Schrœter gives two drawings of Timocharis (*Tab.* XVII. XVIII.). In the first, made when Lambert was near the morning terminator, he shows a small central mountain, the crater *a*, and, very prominently, two white streaks radiating from the southern wall, but no other light markings. These latter features, if they are intended to be more than a conventional indication of the fact that Timocharis is a ray centre, certainly do not exist as conspicuous objects in our time. Though there are faint traces of a ray on the south-east side of the formation, and the surface on the south is involved to a certain extent in the glimmer bordering the eastern side, there is nothing comparable to Schrœter's rays.

The ridges, &c., in the vicinity of Timocharis, with the notable exception of the great Lambert ridge already referred to, are low, and, except when near the terminator, inconspicuous. The principal one proceeds from the foot of the south-west wall towards the anomalous formation Eratosthenes X, and in great part seems to be made up of distinct hills and low mounds.

Schmidt shows seven small craterlets between the south-east border of Timocharis and the crater *a*.

Light-surrounded craters near Landsberg. Of the seven craters

on the south-east of this formation, A, B, *d*, *e*, are encircled by a nimbus, that surrounding the smallest member of the group, *e*, being by far the brightest. The crater *c*, nearly midway between A and B, has a minute craterlet on its northern rim. North of *c* is a small dark crater, with low walls of the same tone as the Mare.

Kempston, Beds: November 20, 1885.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

OLBERS' COMET OF 1815.

Sir,—As the return of this comet will be eagerly expected towards the end of next year, anything relating to it becomes of interest. Some of the remarks made by the discoverer in the *Berliner Jahrbuch* for 1818 will probably, in the present state of astronomical knowledge, amuse those of your readers who have not seen them, and therefore I offer you the following translation of them, in case you think it worth printing. It will be remembered that the *Jahrbuch* in question was published in 1815, and the periodicity of Pons' comet of 1812 (of which the inclination of the orbit is much greater than that of Olbers' of 1815) was first proved by Encke in 1816.

"A second consequence," says Olbers, "which may, in my opinion, be deduced from the nature of the orbits of these two comets [Halley's and his own] is that it is very probable that *there is no planet beyond the orbit of Uranus.* Whatever may be said with regard to the known progression (which is certainly not mathematically exact) of the mean distances of the planets from the sun, and although it has not yet been possible to assign any cause or origin for this progression, yet the inductive proof of it by its correspondence with the orbits of all the known planets is complete, so that we are justified in assuming, with regard to the exterior planets of our system, that the mean distance of each is double that of the preceding. If, therefore, there really were a planet revolving beyond the orbit of Uranus, its mean distance from the sun would be nearly 38 semidiameters

of the earth's orbit, and far beyond the aphelia of the orbits of these two comets. Now there must have been a cause in the formation of our solar system (as Laplace, amongst others, has so clearly pointed out) which produced the general direction of the planetary motions from west to east, the near correspondence of the planes of their orbits with that of the ecliptic, or of the sun's equator, and the nearly circular form of these orbits. Now whether this cause be, as Laplace supposed, a solar atmosphere which formerly extended to the orbits of the exterior planets, or any other whatsoever, no orbits could subsist, or even be produced within the sphere of its influence, which should be so eccentric as are those of these two comets, of which one is inclined at an angle of nearly 44° to the plane of the ecliptic, and the other [Halley's] is described with retrograde motion. It results from this that they must be beyond the sphere of the influence by which the planets were formed and their orbits defined, so that this cannot have extended to so great a distance from the sun as we must suppose a planet revolving beyond the orbit of Uranus to be placed, if such existed."

Is it possible, however, that Olbers may have been in some sense right after all? All the comets of which the aphelia of the orbits are nearer the sun than Uranus move in the same direction as all the planets and the satellites of all those which move within the orbit of Uranus. The comet of least aphelion distance which has a retrograde motion is Tempel's, of 1866, which is now well known to be connected with the November meteoroids; and its aphelian distance is a very little greater than the distance of Uranus. The satellites of Uranus and the satellite of Neptune move in a retrograde direction. Is it possible that Uranus and Neptune were not original members of the system, and were, so to speak, captured by the sun at some remote epoch, when coming within the sphere of its influence? Additional probability is given to this theory, in the case of Neptune, from the fact that its distance does not correspond to Bode's law.

I am, Sir, yours faithfully,

Blackheath: Nov. 18, 1885. W. T. LYNN.

THE PONS' COMET METEORS AND CONTEMPORARY SHOWERS.

The time for meteors that might be connected with Pons' comet is soon coming round again. I give here the principal results of my observations made last year, supplementary to the paper in vol. xxii., p. 305. The weather was very fine, so I watched a good deal every night from the 4th to the 11th

December; but the 12th was overcast. In all I watched for what would be equivalent to 871 minutes in a cloudless sky, and saw 143 meteors, of which nearly half were Geminids. These observations do not throw so much light as I expected on the supposed radiant points given in my paper, it being evident that most of the non-Geminid meteors, unless they are sporadic, are sparse showers that appear very irregularly from year to year. The principal corrections I could make in the list given in the previous paper are these :—

Radiant No. 2 should be struck out, and two of its meteors allotted to No. 3; the third meteor, namely, that on the 4th December, appearing to belong neither to this shower nor to the true Taurids. To No. 3 (Greg's No. 210, R.A. 80°, Dec. + 23°) I now add 7 meteors seen in 1884 between the 8th and the 11th.

One probable Andromede (Radiant No. 8) was seen on the 8th.

There is strong probability of a shower at R.A. 129°, Dec. + 71°, on the 11th, to which I give 4 quick meteors, and 3 of previous years, without, however, considering this radiant really proved.

From the 7th to 9th 6 meteors were seen, probably belonging to No. 14; they are slow meteors of the shower Denning locates at R.A. 208°, Dec. + 71° (*Monthly Notices*, vol. xxxvii., p. 111); the average radiant of these 6 is at R.A. 228°, Dec. + 69°.

To No. 20 I allot 2 meteors on the 11th, and 1 on the 10th; and think there can hardly be a doubt that this is really a radiant, perhaps a sub-radiant of Greg's No. 175, at R.A. 135°, Dec. + 40°. On the 10th 4 other meteors came from nearer Greg's position.

I saw but 1 meteor likely to have been connected with Denning's radiant at R.A. 230°, Dec. + 85°; it was on the 9th; and only 1 (observed on the 8th) for his radiant at R.A. 214°, Dec. + 62°.

There again appeared to be a shower of swift meteors from the neighbourhood of the radiant for Pons' comet, 10 meteors having come from that region, 1 each on 4th and 5th, 2 each on 7th and 8th, and 4 on the 11th. I cannot say they all belong to one radiant point. The average of the 10 is at R.A. 201°, Dec. + 72°; but it seems more likely there are two radiants, as 4 of the meteors might belong to either, it is difficult to ascertain them. Assuming that these 4 meteors, with 3 others, belong to radiant No. 15, they give an average radiant at R.A. 183°, Dec. + 75°; and the remaining 3, with the 2 ascribed in my last year's paper, to Radiant No. 16, give an average radiant point at R.A. 201°, Dec. + 67°. This last is very close to the radiant point for a shower connected with Pons' comet; but

considering that that comet passes so far within the earth's orbit, can it be considered likely for an annual display of meteors to occur from it? If the meteors are supposed to come from the tail, would not their path be hyperbolic? And, therefore, would not they leave the neighbourhood of the earth's orbit when the comet itself did so? Then, too, their orbit would be very different from that of the nucleus. Has this ever been considered in calculating the radiant point?

The average radiant point of the whole 19 meteors probably coming from the neighbourhood of Pons' comet radiant, viz., 10 in 1884, and 9 reported in my paper of last year, is R.A. 205°, Dec. + 73°; but they make a somewhat diffuse radiant, extending over 13° of Dec., but a shorter distance of R.A., except for one meteor.

Yours truly,

Sunderland: 1885, Nov. 18. T. W. BACKHOUSE.

DR. GROTH'S SOLAR SYSTEM, AND HIS PAST AND PRESENT DISTANCE OF THE PLANETS FROM THE SUN.

Dear Sir,—In answer to your critique in No. 275 of the *Astronomical Register*, I beg of you to admit the following short lines, if space permits, in your next issue.

I have carefully considered Dr. J. Croll's and *all* other theories, anent the glacial periods; but cannot admit them to be decisive, as there are many geological facts that cannot be cited here, but which show that a sudden decrease of temperature *must* have arisen over our globe. The most important instance is, the remains of giant tropical herbivorous animals, which are found imbedded over a space occupying hundreds of square miles, in the tertiary and diluvial strata of the northern regions. It is but natural to assume that these animals would, if a gradual decrease of temperature had taken place, have left their abode and found their way to warmer southern latitudes, where they would have found plenty of food, and could not have perished by starvation and cold.

That planets must have receded at least some distance is, I hold, as I have pointed out, beyond question. It is also evident that the sun, with the projection of mass from its surface, must have undergone a slight shifting in its axis of rotation, each time such an occurrence took place; that is to say, a change in direction, which, together with the recession of the planets, accounts for a sudden decrease of heat, and for at least two glacial periods, firstly, at the projection of Mercury, and secondly

at that of Vulcan. The body of the sun could scarcely have been homogeneous, when the planets were projected, and this, as well as the shifting in the sun's axis of rotation, explains again the different inclinations of the planets' orbits.

The causes given for a sudden change of temperature and glacial periods of our planet, will hold good for the cooling and explosion of planet X, his small satellite and small x—*the lost one*—in Saturn's system, which were specially prepared for such a catastrophe.

The disappearance of these bodies took place before our earth was called into existence, and explains the gaps that we have in the Solar and Saturnian systems. In the former, we find the asteroids as fragments, and in the latter only a void space. It is very remarkable that these gaps are there, where we expect to find a planet and a satellite, and that such an immense distance should exist between Titan and Rhea, without any plausible cause; the much more so, since we find a certain order, *even* in the distance of Saturn's satellites, and I must ask, how otherwise this void space can be explained?

The order of formation of planets and satellites can only have taken place as indicated in my pamphlet, and neither your impartial and painstaking criticism, nor Messrs. Proctor and Lockyer's* biassed objections to my theories disprove them, and I must uphold almost everything I have said until the contrary is satisfactorily proved.

Yours truly,
ERNEST GROTH, M.D.

5, Weymouth Street,
Portland Place, W.: November, 1885.

LONGITUDE of the MOON'S TERMINATOR at MIDNIGHT.

N.B. — means East. + West. M., Morning Terminator.
E., Evening Terminator.

1885.	° ′		° ′		° ′
Dec. 1	−30 47 E.	Dec. 11	+27 37 M.	Dec. 21	+86 2 E.
2	42 57	12	15 27	22	73 52
3	55 7	13	3 17	23	61 43
4	67 16	14	− 8 52	24	49 33
5	79 26	15	21 2	25	37 24
6	+88 25 M.	16	33 11	26	25 14
7	76 15	17	45 21	27	13 5
8	64 5	18	57 30	28	0 56
9	51 56	19	69 39	29	−11 13
10	39 46	20	81 48	30	23 23
				31	35 33

Moon farthest from the Earth, Dec. 10, 10h.
,, nearest the ,, ,, 22, 13h.

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* As Editors of *Nature* and *Knowledge*.

THE PLANETS FOR DECEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	18 3 31	S. 25 39	6″·6	1 21·3
	9th	18 23 31	S. 24 14	8″·2	1 9·7
	17th	17 59 3	S. 21 52	9″·8	0 13·9
	25th	17 16 40	S. 19 58	9″·0	22 56·3
Venus ...	1st	19 57 21	S. 23 29	22″·8	3 14·8
	9th	20 32 5	S. 21 20	24″·8	3 18·0
	17th	21 3 51	S. 18 44½	27″·4	3 18·2
	25th	21 32 3	S. 15 50	30″·2	3 14·9
Mars ...	1st	10 54 22	N. 9 22½	8″·4	18 9·4
	9th	11 7 0	N. 8 15½	8″·8	17 50·5
	17th	11 18 23	N. 7 16	9″·4	17 30·4
	25th	11 28 21	N. 6 25	10″·0	17 8·9
Jupiter ...	1st	12 10 54	N. 0 7	32″.2	19 25·7
	9th	12 14 40	N. 0 15½	32″·8	18 58·0
	17th	12 17 52	N. 0 34	33″·6	18 29·7
	25th	12 20 29	N. 0 49	34″·4	18 0·9
Saturn ...	1st	6 29 31	N. 22 23½	18″·4	13 45·2
	9th	6 27 0	N. 22 25½	18″·4	13 11·3
	17th	6 24 17	N. 22 28	18″·6	12 37·1
	25th	6 21 28	N. 22 13	18″·6	12 2·9
Neptune ...	3rd	3 27 17	N. 16 59		10 35·4
	19th	3 25 29	N. 16 53		9 30·9

Mercury sets about an hour after the sun, at the beginning of the month, the interval slightly increasing for a few days, and afterwards rapidly decreasing. Towards the end of the month he rises before the sun, the interval increasing to an hour and three quarters by the last day.

Venus sets about 7 p.m. on the 1st, and slightly later each night afterwards.

Mars rises three-quarters of an hour before midnight at the beginning of the month, the interval increasing.

Jupiter rises about an hour and a quarter after midnight on the 1st, the interval decreasing.

Saturn rises on the 1st about an hour and three-quarters after sunset, the interval decreasing.

THE MINOR PLANET VESTA.

The minor planet Vesta, the fourth in the series, will come into opposition on the 9th of the month.

Date.	R.A. h.	m.	s.	Dec. °	′	″
Dec. 7	5	14	43·36	N. 18	1	42
9	5	12	30·20	N. 18	3	59
11	5	10	16·47	N. 18	6	24

ASTRONOMICAL OCCURRENCES FOR DECEMBER, 1885.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. α Persei.
Tues	1		Sidereal Time at Mean Noon 16h. 42m. 1·45s.			10 32·4
Wed	2	16 24 16 56	Occultation of κ Virginis (4½) Reappearance of ditto	2nd Ec. D. 2nd Oc. R.	13 53 35 19 0	10 28·5
Thur	3	12	Mars at quadrature with the Sun	3rd Sh. E. 3rd Tr. I.	15 37 17 4	10 24·5
Fri	4		Sun's Meridian Passage 9m. 28·29s. before Mean Noon	2nd Tr. E. 1st Ec. D.	14 8 19 3 2	10 20·6
Sat	5			1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E.	16 9 17 19 18 26 19 35	10 16·7
Sun	6	1 16	● New Moon	1st Ec. D. 1st Oc. R.	13 31 17 16 56	10 12·7
Mon	7	18	Conjunction of Moon and Mercury 6° 3′ S.	1st Tr. E.	14 4	10 8·8
Tues	8					10 4·9
Wed	9			2nd Ec. D. 4th Tr. I. 4th Tr. E.	16 27 52 17 26 19 30	10 0·9
Thur	10	11	Conjunction of Moon and Venus 5° 56′ S.	3rd Sh. I. 3rd Sh. E.	16 20 19 33	9 57·0
Fri	11	8 31	Occultation of 18 Aquarii (6)	2nd Tr. I. 2nd Sh. E. 2nd Tr. I.	13 59 14 24 16 47	9 53·1
Sat	12	5 56 6 56	Occultation of B.A.C. 7697 (6½) Reappearance of ditto	1st Sh. I. 1st Tr. I.	18 2 19 14	9 49·1
Sun	13			1st Ec. D. 1st Oc. R.	15 24 25 18 51	9 45·2
Mon	14	6 21	☽ Moon's First Quarter Saturn's Ring: Major axis=46″·51 Minor axis=20″·27	1st Tr. I. 3rd Oc. R. 1st Sh. E. 1st Tr. E.	13 43 14 29 14 47 15 59	9 41·3
Tues	15		Sidereal Time at Mean Noon 17h. 37m. 13·24s.	1st Oc. R.	13 19	9 37·4
Wed	16	13 47	Occultation of μ Piscium (5) Sun's Meridian Passage 3m. 59·22s. before Mean Noon	2nd Ec. D.	19 2 18	9 33·4
Thur	17		Occultation of B.A.C. 741 (6½)	4th Ec. D. 4th Ec. R.	16 36 46 19 23 38	9 29·5
Fri	18	11 2 12 2 16	Occultation of B.A.C. 987 (6½) Reappearance of ditto Inferior conjunction of Mercury and Sun	2nd Sh. I. 2nd Tr. I. 2nd Sh. E. 2nd Tr. E.	14 7 16 36 16 57 19 24	9 25·6

DATE.		Principal Occurrences. h. m.		Jupiter's Satellites.	h. m. s.	Meridian Passage. h. m.
Sat	19	16 11	Near approach of γ Tauri (4)	1st Sh. I.	19 55	*α* Persei. 9 21·6
Sun	20	5 6 17 6 17 56 18 35	Near approach of B.A.C. 1526 (6) Occultation of 111 Tauri (5½) Reappearance of ditto Occultation of 117 Tauri (6)	2nd Oc. R. 1st Ec. D.	13 35 17 17 31	9 17·7
Mon	21	8 58 5 46 6 37 17	○ Full Moon Occultation of B.A.C. 1930 (6¼) Reappearance of ditto Conjunction of Moon and Saturn 3° 58′ N.	3rd Ec. R. 1st Sh. I. 3rd Oc. D. 1st Tr. I. 1st Sh. E. 1st Tr. E. 3rd Oc. R.	13 34 42 14 23 15 35 15 38 16 40 17 54 18 30	9 13·8
Tues	22			1st Oc. R.	15 14	9 9·8
Wed	23			1st Tr. E.	12 22	9 5·9
Thur	24	15 55 17 5	Occultation of ξ Leonis (6) Reappearance of ditto			9 1·9
Fri	25	18 50 19 48 23	Occultation of 48 Leonis (6) Reappearance of ditto Saturn at opposition to the Sun	2nd Sh. I. 2nd Tr. I. 3rd Sh. E.	16 40 19 11 19 31	8 58·0
Sat	26	17 48 18 47 2 21	Occultation of τ Leonis (5) Reappearance of ditto Jupiter at quadrature with the Sun Conjunction of Moon and Mars 2° 48′ N.	4th Tr. I. 4th Tr. E.	12 9 13 20	8 54·1
Sun	27	21	Conjunction of Moon and Jupiter 0° 5′ S.	2nd Oc. R. 1st Ec. D.	16 10 19 10 33	8 50·2
Mon	28	0 21 17 48 18 47 16	☾ Moon's Last Quarter Occultation of θ Virginis (4½) Reappearance of ditto Uranus at quadrature with the Sun	3rd Ec. D. 1st Sh. I. 1st Tr. I. 3rd Ec. R. 1st Sh. E. 3rd Oc. D. 1st Tr. E.	14 31 54 16 16 17 31 17 31 49 18 34 19 34 19 47	8 46·2
Tues	29			1st Ec. D. 1st Oc. R.	13 38 49 17 7	8 42·3
Wed	30			1st Tr. I. 1st Sh. E. 1st Tr. E.	11 59 13 2 14 15	8 38·4
Thur	31	17 8 18 15	Occultation of η Libræ (6) Reappearance of ditto			8 34·4